



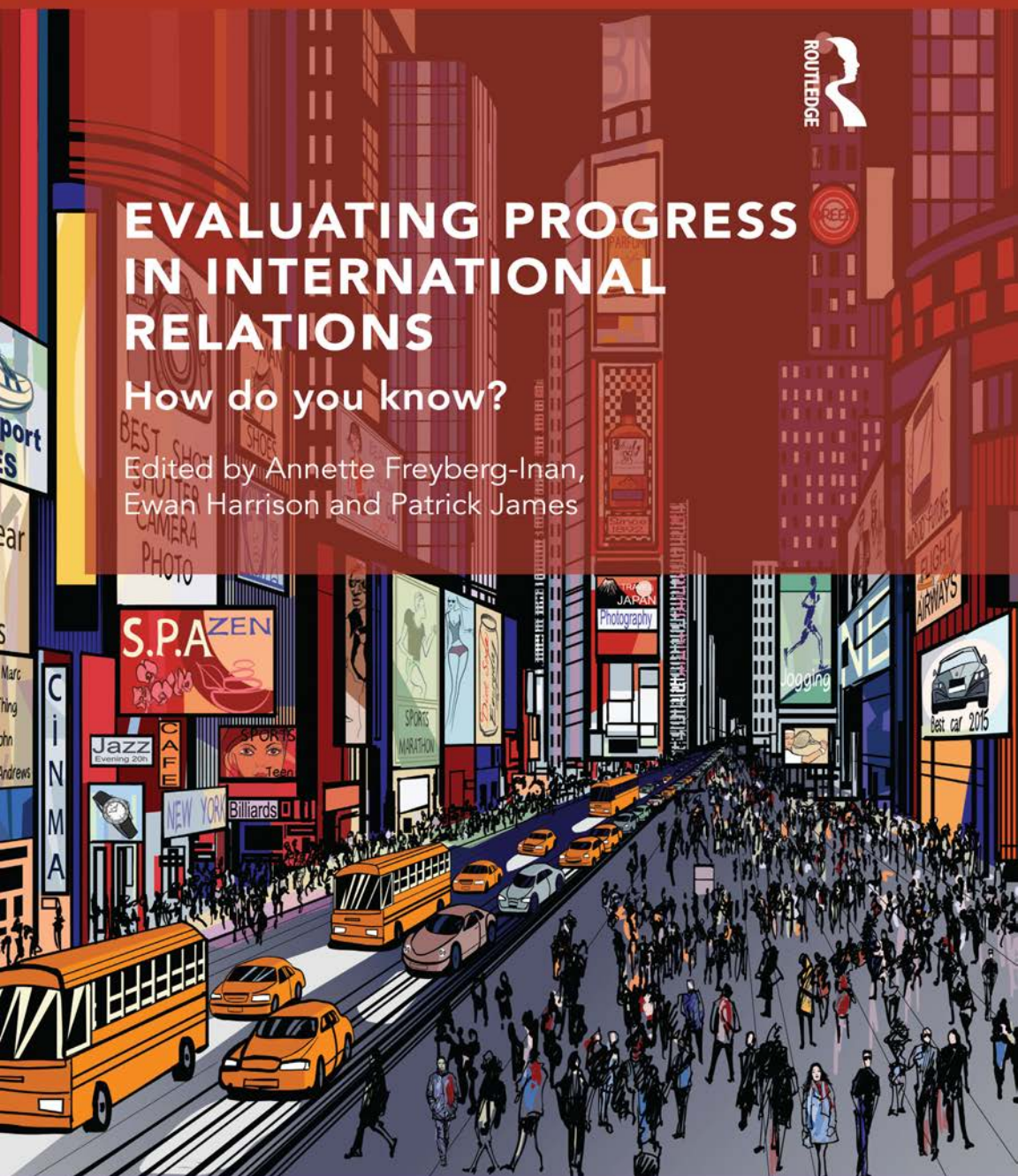
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Ewan Harrison is Assistant Professor in the Department of Political Science at Rutgers University, USA.

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Annette Freyberg-Inan and Ewan Harrison dedicate this book to their co-editor Patrick James in sincere gratitude for being a truly outstanding scholar, mentor, and friend over two decades.



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Routledge, 2010), *Agents, Structures and International Relations* (Cambridge, 2006) and *Realism, Philosophy and Social Science* (authored with Kathryn Dean, John Roberts, and Jonathan Joseph, Palgrave, 2006). He was Editor in Chief of the *European Journal of International Relations* from 2008–2013.

Foreword

Although there have undoubtedly been thinkers who have pondered on the essential characteristics of international relations for several millennia, there is considerable dispute about when International Relations began to emerge as a formal academic discipline. Some have argued that the field only developed a distinctive presence in universities after World War II, whereas others now insist that the origins can be traced back to the nineteenth century. This disagreement has served to complicate the conventional historiography of the field that suggests the discipline has evolved through a series of unresolved “great debates,” starting with the dispute between realists and idealists in the period between World Wars I and II. This quarrel is then seen to have given way to a methodological debate between behavioralists and traditionalists, which in turn gave rise to an inter-paradigm debate. There is, however, something more than slightly suspect about this historiography because these so-called debates all have deep roots and reflect the fact that the study of international relations has always been controversial across a range of different fronts. Given this assessment, it is unsurprising that there should be desire among some within the field to transcend these controversies and identify an approach whereby the work of different scholars can generate knowledge that accumulates rather than pulls in different and competing directions. This is certainly the aim of the contributors to this book.

It is not difficult to infer from this aim that the contributors, or at any rate most of them, wish to find an approach that will allow International Relations to emulate the progress that has been made in the natural sciences. At the same time, these scholars are well aware that this is not an easy task. While it is very difficult, if not impossible, to question the fact that the natural sciences have made constant and continuous progress across recent centuries, historians of science have never been able to agree on how to chart this progress and there is certainly no agreement among philosophers of science about how natural scientists have managed to ensure that scientific knowledge has accumulated so consistently and on such a consensual basis. Nevertheless, this desire to find a route that will allow the development of accumulating knowledge is certainly not peculiar to specialists in international relations. It is common to all social scientists, for pragmatic as well as academic reasons, and in particular, perhaps, to

overcome the popular assumption that the very idea of a social science is an oxymoron. This assumption was often claimed to underlie the decision in 1979 in Britain by Margaret Thatcher's newly formed Conservative government to reassess whether to continue state funding for the social sciences through the Social Science Research Council. After a hard-fought battle, funding was continued but it is significant that the funding body was relabeled the Economic and Social Research Council. The reference to science was dropped. Does the change reflect the prejudice of a prime minister who had become a researcher after completing a degree in chemistry? To this day the funding of pure research in the social sciences remains precarious in Britain.

There is, of course, the alternative prejudice of those social scientists that have claimed that natural scientists were given the easy problems to solve and the really hard problems have been left for the social sciences to wrestle with. Both prejudices, however, reflect a facile and unconvincing view of the world – both natural and social. Indeed, it is increasingly clear that the separation of natural and social sciences rests on a false dichotomy and so neuroscientists, for example, are now showing how the brain has been wired in a way that makes it possible for individuals to interact within a social arena.

But the fact remains that the social sciences in general and International Relations in particular lag far behind all of the natural sciences when measured in terms of the progress that has been made in developing universally accepted knowledge. For some, however, this is a necessary and acceptable state of affairs because for a number of reasons the social sciences necessarily deal with value laden and essentially contested concepts. This is itself a feature of the social world and so it must also of necessity be a feature of the analysis of this world. Or so it is argued. But others, and this includes most of the contributors to this volume, do not accept this conclusion and remain convinced that it is possible and indeed essential for social scientists to draw on the philosophy of natural science to refine our understanding of how we must carry out research in International Relations. And there is a conviction that in doing so it will be possible to make progress akin to the progress made by natural scientists.

The overall orientation of this book, however, is neither rigid nor didactic. Indeed, the editors have necessarily maintained a flexible and open mind because they have endeavored to ensure that they have embraced all the major approaches to the question of how to promote a consensual approach to the development of knowledge in International Relations. In doing so, they have drawn together some of the most influential thinkers in this field and unsurprisingly there are some major differences among them. But what the book does very effectively is to tease out both the differences and the similarities in these approaches. A key device used to achieve this end is to explore the research generated by the democratic peace theory, one of the very few research programs in International Relations where there is widespread agreement that discernible progress has been made.

What the book ultimately succeeds in doing, therefore, is finding a formula which provides enough common ground between the divergent positions

developed in the book to make the claim that there is a way of generating an intersubjective consensus among these positions thereby permitting what Freyberg-Inan calls sociable pluralism. Not everyone is going to accept this assessment but even those who dissent must accept that the book succeeds in generating a very clear articulation of the different ways of achieving progress in the field of international relations and it will be required reading for anyone interested in this question.

Richard Little
University of Bristol

Preface

This volume is the product of an International Studies Association (ISA) “Venture Workshop” grant, jointly awarded to the three co-editors, along with Laura Sjoberg of the University of Florida and Samuel Barkin of the University of Massachusetts, Boston. One of the largest workshop grants ever offered by ISA, this support launched our project at the ISA convention in Toronto in the spring of 2014. Attended by about 20 major scholars from around the world, Sjoberg and Barkin’s side of the Venture Workshop focused on methodological fragmentation within the field and engaged exponents of critical and constructivist theories with complex and sophisticated qualitative techniques for analysis. It has resulted in a separate edited volume (Barkin and Sjoberg, forthcoming). By contrast, our side of the workshop and associated edited volume focused on epistemological rather than methodological debate within IR, and specifically on the application of philosophy of science in the profession.

The present volume also builds substantially on our previous successful collaboration as co-editors. Our 2009 edited volume, *Rethinking Realism in International Relations*, has become a reference work for scholars interested in progress in realist IR. It considered which varieties of realist IR theory showed most promise for the future development of both theoretical utility and empirical insight in our field – and why. Similarly to the current volume, it included different voices ranging from mainstream positivist to critical and post-positivist. This book may be considered a sequel to *Rethinking Realism* in that it debates broader and more varied notions of progress at an epistemological level and with application to a specific empirical research program (the democratic peace) as opposed to a broad theoretical paradigm.

Patrick James is grateful to his extraordinary co-editors for their efforts throughout the process of putting together this volume. Indira Persad from the Center for International Studies at USC should be singled out for assistance at many points as the contents of this book came together. Annette Freyberg-Inan wants to thank her co-editors and our authors as well as the University of Edinburgh, Scotland for the IASH-SSPS Visiting Research Fellowship during which she was able to do most of her work on this volume. We all express our deep gratitude to the International Studies Association for providing our workshop grant as well as a long-term intellectual home. Ewan Harrison would very much

like to thank Laura Sjoberg for the help and advice in drafting the ISA Venture workshop proposal. Finally, all three editors are grateful to participants at the ISA workshop, which launched this volume through a first-rate exchange of ideas.

Patrick Thaddeus Jackson notes that an early version of some of the components of the arguments presented here has been part of his course “Knowing and the Known” in the IPSA’s Summer School in Sao Paulo, Brazil in 2013, 2014, and 2015, and in the ECPR’s Winter School in Vienna, Austria in 2013 and 2014. He thanks the student participants in those courses for their indulgence and their feedback. Working versions of the argument were also presented at the University of Oxford, Aberystwyth University, the University of St Andrews, and Cornell University in 2013. Patrick offers special thanks to Adam Freeman, whose discussant comments at Oxford were extremely helpful to sharpening his thinking. An ISA workshop in 2014 and a panel at the ISA-Northeast in 2014 were also useful for discussion of his chapter.



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Introduction

Progress, consensus, and cumulation in IR scholarship?

Ewan Harrison, Annette Freyberg-Inan, and Patrick James

Social scientists appropriately produce social science. There is no need for them to defend positions and advance debates in the philosophy of social science. It is up to philosophers to do so. However, it is essential for social scientists to have enough training in the latter to be able to understand their own methods and to advance their own substantive area so that they can develop their ... arguments within an intellectually coherent framework.

(Fred Chernoff, *Explanation and Progress in Security Studies*, 2014: 267)

The study of International Relations (IR) has undergone epistemological fracturing. Understandably, most scholars either have not been particularly interested in or know little about philosophy of science. Instead, they have concentrated on “doing” their research. Even when philosophy of science criteria are utilized, they often have been used implicitly or poorly. The result is that powerful available tools for facilitating dialogue and exchange between researchers have been underexploited, or worse, ignored. IR has not been making use of resources that are critical to effective self-evaluation and productive exchange of ideas or disagreement. The profession virtually ignores a crucial asset of collective knowledge production and unfortunately many scholars remain content with this. Not enough scholars appreciate why they are doing research, to what larger stock of knowledge they contribute, whether and how their work is persuasive to their peers, and what the profession is amassing (Chernoff, 2014; Jackson, 2011). The field has a major problem that cries out to be addressed. IR lacks tools for scholars with different epistemological orientations and training to speak to each other clearly, and to understand, fairly evaluate, and even *use* each other’s insights. The growing size of the international studies research community has often not produced a symphony, but a cacophony.

This book offers a systematic evaluation of how knowledge is produced by scholarly research in IR. What are the different approaches to defining the “scientific method” in application to IR? To what extent is scientific progress and accumulation of knowledge possible? What are the different accounts of how this occurs? And what are the dominant critiques of these positions? What are the domains within which researchers in IR claim to have achieved scientific

progress? One area that might be cited as a model of scientific progress is inquiry into the democratic peace, or the absence of war between liberal democratic states (Doyle, 1983a; Maoz and Russett, 1993). Yet can the study of the democratic peace act as a paragon for scientific progress in IR? What disagreements are there within the field about the past and future trajectory of democratic peace research and what is their significance? This book's objective is to increase understanding of the sophistication and complexity of these debates and their importance for the conduct of inquiry in the field's major research programs. We aim to provide an accessible "road map" for researchers and students negotiating this epistemological minefield. Ours is the first book to survey the full range of perspectives available for evaluating scientific progress, as well as dominant critiques of scientism. It offers a unique key guide to these increasingly salient discussions.

Our aim is to cut through confusion and allow scholars and students to understand and use tools available from debates in the philosophy of science better and more easily. We do not seek to "push" one particular position or line. Here there is a difference between our approach and some of the more conventional or "hard positivist" takes, such as that offered in Elman and Elman's (2003) volume based on Imre Lakatos's influential *Methodology of Scientific Research Programs* (MSRP). The importance and suitability of such conventional approaches is acknowledged, engaged with, and identified in our book from a variety of angles. Indeed, the centrality of Lakatosian methodology, and more broadly of mainstream positivist approaches to current debates in IR, is emphasized (James, 2002b; Harrison, 2003). However, we also consider a range of soft positivist and non-positivist approaches, and show how these perspectives offer valuable alternatives for understanding IR. We firmly believe that this type of interchange increases the scope for deepening mutual understanding and the possibility of cross fertilization of ideas in the profession. We hope our book provides a model for how this kind of constructive conversation can take place across the positivist/non-positivist divide.

This volume developed in the critical space opened up by two influential recent contributions to debates about the philosophy of science and its role in IR. That two such influential analyses have been published just three years apart is a hopeful sign that the field as a whole is at last engaging in visible debates about epistemology. The most recent of the books is Fred Chernoff's *Explanation and Progress in Security Studies* (Chernoff, 2014). While Chernoff himself advocates methodological pluralism (2014: 24; see also, Chernoff, 2007a), his book is valuable also for providing a comprehensive account of the positivist tradition. According to the positivist view, methods from the natural sciences are directly applicable to studies of the social world. Law-like regularities and general patterns of behavior can be identified by researchers, and these may be examined with reference to causal explanations. Competing hypotheses regarding causality may be tested against the evidence on the basis of a free, fair, and objective competition, and over time a consensus will emerge as toward which causal accounts are the most accurate (Chernoff, 2014: vii). While these criteria are stringent as

well as contested, they have been applied to research within IR and, at least for the democratic peace research program, arguably have contributed to achieving a sense of scientific progress. The chapter by Chernoff in this volume develops this argument further.

Patrick Jackson's *The Conduct of Inquiry in International Relations* (Jackson, 2011) has provided a comprehensive account of both neopositivism and three alternatives to it, which he names critical realism, analyticism, and reflexivity. His book has already established itself as one of the leading texts dealing with epistemological debates in IR and is set to make a lasting impact on research and discussion. His chapter in this volume develops his take further. Jackson is keen to demolish the neopositivist account of science. His argument begins from the assertion that social science is just that – *social* not natural science. Due to differences in subject matters studied across the natural and social worlds, there is not and will never be one single definition of science in the social scientific arena, including IR. Instead, there are four different types of social science, each equally “scientific” and legitimate on its own terms. Alternatives to neopositivism emphasize unobservables and/or the mutually constitutive interaction between subject and object in social scientific research.

Each of Jackson's four categories of social science rests on certain assumptions or “wagers” (Jackson, 2011: 32) which are mutually incommensurable. Thus there is no need to resolve philosophical disagreements about the nature of “science.” Social scientific researchers need to be aware of the assumptions they make, of the limitations of their own approach to science, and appreciate how their work stands in relation to other social scientific expositions. IR needs to be mature and pluralistic enough to operate with different epistemologies that coexist. There is not one epistemological yardstick but four. This has major implications for how IR understands scientific progress, consensus, and cumulation. Jackson implies that IR should not have particularly high hopes for these goals. To the extent they are achievable, this biases the field toward a narrow neopositivist conception of science that many other researchers in the field reject or at least problematize. For that reason, Chernoff criticizes Jackson's approach for offering researchers no guidance either on how to choose between competing conceptions of scientific method or rationally deciding which approaches to science are better than others at generating cumulative knowledge (Chernoff, 2013a: 352).

Chernoff and Jackson offer powerful statements that have crystallized and synthesized important arguments, and which have already become landmark points of reference. We see our volume as contributing to a conversation they have opened up regarding how to combine systematic analysis of the criteria we use to judge progress in our field with an openness to what both call methodological pluralism. To that end, we explore the value of what we term “sociable pluralism” and demonstrate its potential benefits for researchers. The epistemological position of sociable pluralism is one that seeks to build bridges between and among soft positivism, critical theory, and critical realism. We hold that sociable pluralism represents the cutting edge in social scientific thinking about

knowledge production without slipping into dogmatic extremes that disallow productive conversations with scholars of different persuasions. Moreover, sociable pluralism also encourages and invites scholars skeptical about positivism to engage with debates about scientific progress and evaluation of it by more “mainstream,” hard positivist perspectives.

Sociable pluralism is to be distinguished from methodological pluralism as defined by either Chernoff or Jackson. It explores boundaries and connections across different possible approaches in a way that is cognizant of their deep differences yet also open to cross fertilization and mutual exchange. In this way we hope to guide readers to make more informed choices and reach more sophisticated understandings of complex epistemological questions and debates. Our approach is ecumenical, allowing scholars and students to evaluate for themselves the subtleties and ambiguities of different perspectives in a back-to-back comparison, as well as to come to a greater appreciation of the nuanced complexities of the stakes. Our reasoning is that, if the differences between the various approaches and metrics used by communities of researchers can be understood more clearly, this will facilitate meaningful cross-cutting communication, dialogue, and debate.

The starting point for this volume is an observation that social sciences generally, and IR more particularly, find themselves under increasing societal and political pressure to justify their existence and material support. This creates a need to demonstrate that we as scholars, collectively speaking, are doing something useful. Traditionally, attempts at self-justification in the (social) sciences have been tied to notions of both “science” and “progress,” which makes inescapable an engagement with these notions and the criteria that have been developed for identifying science, progress, and their corollaries (such as the ability to predict and control) in scholarly work.

The first half of this volume (1) unpacks and problematizes these notions and (2) shows the range of possible and legitimate responses to the challenge of demonstrating our usefulness that fall within the paradigm of sociable pluralism. The second half of the volume applies this range of approaches to one specific research program. We use the research program on the democratic peace, which is considered one of the most scientifically developed research programs in IR (if not *the* most scientifically developed) to show what we gain by accommodating and enabling dialogue among the full range of epistemological approaches open to sociable pluralism. It becomes evident that, rather than inviting a cacophony of incoherent research efforts, an embrace of sociable pluralism is essential for building consensus behind theoretical claims.

Our volume assembles preeminent scholars in the philosophy of science of IR to offer a guide to the state of the art in this increasingly important and yet vexing subfield. All chapters explicitly address three core questions: What criteria exist, and which should be advocated, for evaluating contributions to knowledge? How can we best utilize those criteria for evaluating past and future theoretical contributions? And what are the implications of these criteria for progress in IR? While the chapters in the first part address these questions at a

general level, the chapters in the second part provide concrete applications to what often is considered IR's strongest research program.

Competing definitions of “science” and “scientific progress” in IR

There is a growing demand from scholars and students for help to navigate the complex conceptual minefield that debates about science and scientific progress have become. These contentions have emerged as the field has matured and undergone increasing professionalization and specialization. The books by Chernoff (2014) and Jackson (2011) have opened up a vista of perspectives on judging scientific value and potential for progress in IR. While the former comes from a position closer to positivism, both volumes support methodological pluralism. Two other volumes, one by Patrick James and the other by Colin and Miriam Elman, also stand out as landmarks in the literature on philosophy of science and IR (James, 2002b; Elman and Elman, 2003; see also Harrison, 2003). Operating within the positivist tradition, these studies both utilized Imre Lakatos's famous MSRP to evaluate and assess scientific progress across a series of major research programs within IR (Lakatos, 1970). However, these books were weaker at engaging non-positivist perspectives and at considering problems and ambiguities in the application of Lakatosian methodology to IR. There is a serious paucity of books that evaluate debates on scientific cumulation and progress from both positivist and non-positivist perspectives. Our volume seeks to fill this gap and offer a comprehensive and balanced survey of arguments in all epistemological branches of IR.

Each chapter that follows engages with and answers three levels of questions. The first and deepest level of questions asks how, if at all, we judge progress and accumulation of knowledge within the field. What tools or criteria are available from philosophy of science to do so? Chernoff, for example, engages a broader positivist (or to Jackson, “neopositivist”) approach in the study of IR going back to Keohane's seminal work applying Lakatosian methodology (Keohane, 1986). This approach models IR on the natural sciences and seeks to identify and explain causal laws of behavior and locate the most persuasive account of patterns of behavior by testing competing hypotheses against empirical evidence. Through competition between scientific hypotheses, the best explanation emerges, scientific progress occurs, and knowledge accumulates. This argument subsequently has been developed by scholars such as Vasquez (1999), James (2002b) and the Elmans (2003). By contrast, Jackson operates within a tradition going back to Hollis and Smith's influential work (Hollis and Smith, 1990), further developed by Moses and Knutsen (1998). This approach, based on the distinction between “explaining” and “understanding,” distinguishes more than one “way of knowing” (to use Moses and Knutsen's idiom). In contrast to positivism, interpretivist or historicist accounts (1) see evidence as being deeply laden by observer and other biases; and (2) favor descriptive or hermeneutic over causal explanation.

Moving down the ladder of epistemological abstraction, a second level of questions relates to what tools from the philosophy of science can achieve in evaluating IR research. The positivist answer to this question is that various sets of metrics drawn from debates in the philosophy of science can inform evaluation of empirical work in the social sciences. When scholars are publishing books and articles explaining different aspects of world politics they are not operating in isolation. These scholars instead are creating a body of knowledge which is evolving, is structured and coherent, and which may be systematically measured and compared at different points in time. Chernoff identifies at least five different approaches or metrics drawn from the philosophy of science (Chernoff, 2004, 2007a). Yet what he develops most systematically is the “conventionalist” approach offered by Pierre Duhem (Duhem, 1954; Chernoff, 2014). According to this perspective, the key distinguishing feature of science is to offer an “approach to consensus.” Gradually a scientific consensus will emerge in which participants within scientific debates reach agreement over which explanation is more accurate. At this stage, scientific progress has been achieved and can be identified.

The Elmans, by contrast, favor the more widely utilized Lakatosian metric, in which different theories subsume the insights of predecessors, account for new empirical content, and yet remain compatible with earlier theoretical axioms (Elman and Elman, 2003; James, 2002b; Vasquez, 1999; Keohane, 1986). This raises questions about why we should adopt one set of criteria drawn from debates in the philosophy of science rather than another (Jackson, 2011: 11). Moreover, as the ensuing debate in this volume (perhaps most directly between Harrison and Chernoff) illustrates, it is far from clear that these criteria will reach a single “objective” answer, even when applied to the evolution of knowledge within the same research program. This challenge highlights the way in which techniques premised on the natural sciences are not easily transposable into the social world and remain acutely exposed to bias and competing interpretations (Jackson, 2013).

Finally, on the third level of questions, we ask which areas or subfields within IR, if any, have displayed progress and why. How much consensus exists within different parts of the field, and for what reasons? To allow for a focused exchange we concentrate on evaluating research on the democratic peace. Chernoff (2014) has mounted a defense of the argument that the democratic peace research program has generated scientific progress and cumulative knowledge. This argument reinforces earlier studies that have made a similar claim (Ray, 2003; Chernoff, 2004; Ungerer, 2012). Yet this conclusion is critiqued in the pages that follow, from at least two different angles. Operating within a positivist framework, Harrison uses Lakatosian methodology to argue in Chapter 7 that while the democratic peace research program has been progressive in the past, it has begun to stagnate and risks becoming degenerative. The dyadic proposition is so well established that any further knowledge about it will be incremental, with likely diminishing returns. Only by expanding the scope and significance of the claim to the system level of analysis will it be possible to

maintain progress in democratic peace research. This point is reinforced by Hayes and James, who argue in Chapter 6 for a diagrammatic review of the democratic peace to identify its accomplishments and priorities across levels of analysis and set the stage for the most effective further research. Moreover, in Chapters 8 and 9, Ish-Shalom and Sjoberg deeply problematize both Chernoff's and Harrison's competing positivist accounts (see also Ish-Shalom, 2013; Sjoberg, 2013). Together, Harrison, Ish-Shalom, and Sjoberg chip away at Chernoff's argument about a consensus on scientific progress in IR and problematize the intersubjectivity of his claims.

Over the last 15 years, these three levels of questions have been the subject of increasing debate within the field. In the following pages we discuss contributions that serve as points of reference for the current terms of the debate and frame also the contributions in this volume. The most recent, and in some respects the most ambitious, contribution has been provided by Chernoff's previously mentioned *Explanation and Progress in Security Studies*, which at this stage is worth fleshing out in more depth (Chernoff, 2014). Chernoff's starting point is influenced by Thomas Kuhn's analysis in his classic study *The Structure of Scientific Revolutions* (Kuhn, 1970). Kuhn famously argued that scientific inquiry develops within "paradigms," sets of governing assumptions that structure the questions that scientists investigate. Chernoff picks up this idea and argues that his book

begins with the presumption that ... scientists ... conduct research in ways that fit the paradigm ... in which they were trained. They are taught which works are the key works, or "exemplars," in the field.... Students internalize the methods and assumptions of exemplar works and then carry them forward in their own studies.

(Chernoff, 2014: 3)

This also means that by conducting a comprehensive descriptive analysis of the exemplar works in a field, a reliable picture can emerge of what is generally accepted as knowledge within it.

From this insight, Chernoff builds his account of the cumulation of knowledge within security studies. He surveys three prominent arenas for research: nuclear proliferation, alliance formation, and the democratic peace. Each of these research programs has accumulated a voluminous literature. Chernoff identifies "exemplar works" within each vast body of inquiry using three techniques: a survey of the most cited articles in five top scholarly journals; a survey of the editors of the top 20 IR journals (as rated by the Teaching, Research and International Policy, or TRIP, survey); and course syllabi from the top 20 ranked institutions identified in the TRIP survey (Chernoff, 2014: 5). Within each of the research subfields, he uses these sources to identify ten exemplar works. Chernoff finds that in research on the causes of nuclear proliferation, there is a low level of consensus among scholars. There is a major gulf between realist and non-realist explanations of proliferation and neither side has been able to convince the other to abandon its

claims. Thus little scientific progress can be said to have been achieved (2014: 116–122). In the case of research on alliance formation, the situation is somewhat better in the sense that there is “a general consensus that strict balance-of-power explanations should be rejected” (2014: 245). However, scholars remain divided over whether balancing or bandwagoning is the predominant tendency (2014: 174–180). Thus there is only a moderate approach to consensus in relation to the balance-of-power.

In contrast, the democratic peace research program provides grounds for optimism about the possibility of scientific progress in security studies. Here a strong degree of approach to consensus has been reached around the dyadic explanation that pairs of democratic states do not fight wars (Chernoff, 2014: 233). Moreover, liberal, as opposed to realist, explanations of this empirical pattern have been accepted as superior, and are rarely challenged. Debate has progressed to ask more fine-grained questions about which liberal variable (trade, structure, norms) carries the most explanatory power (2014: 236–240). Chernoff takes this as clear evidence in support of scientific progress and the expansion of knowledge within security studies in a manner that replicates the approach to consensus common in the natural sciences. The advice that Chernoff extrapolates is for scholars to be clear and explicit about what constitutes an explanation. Doing so facilitates communication and dialogue between scholars by allowing them over time to accept when and under what circumstances they have been proven wrong. Explicit statements of explanatory criteria overcome obstacles to scientific progress (2014: 266–268).

Patrick Jackson’s *The Conduct of Inquiry in International Relations* (Jackson, 2011) offers a different approach. Jackson’s goal is to reclaim “science” from the neopositivists who have colonized its meaning as applied to the social sciences. Whereas Chernoff seeks clarity, precision, and scholarly agreement, Jackson highlights the inevitability of ambiguity and disagreement about the meanings of science and social science. Jackson holds that there is no consensus about the scientific method even in the natural sciences, let alone in the social sciences. It is not possible, as positivists wrongly assume, to extrapolate a clear definition of science from debates in the philosophy of science. Instead, science can only be defined in relation to what researchers actually practice. He therefore follows Weber in defining social science as “systematic production of factual knowledge about ... social and political arrangements” (Jackson, 2011: 20–22).

This broad definition of science is compatible with various types of enterprises. Jackson argues that four ideal types of epistemologies are adopted in IR. Here there is a commonality with Chernoff’s approach, in the sense that Jackson identifies these ideal types with reference to a descriptive survey of actual research being published in IR. However, Jackson includes research published in the field which does not fall in the positivist and naturalist traditions. He casts his net much broader than the “top” journals, institutions, and syllabi, and on a much wider (non-North American) geographic scale. Indeed, Jackson could argue that the reason Chernoff is able to reach such apparently clear conclusions about how scientific progress can be obtained in IR is that he has surveyed only

narrow pockets of the discipline which, far from exhausting the meaning of science, suit the particular interests of a limited range of U.S.-American IR scholars at “top ranked” universities.

Jackson’s (2011) four ideal types of science are: neopositivism, critical realism, analyticism, and reflexivity. Neopositivism is the dominant approach in the field and seeks to uncover causal laws of behavior in the social world through testing of hypotheses. This is the perspective emphasized by Chernoff and associated also with King, Keohane, and Verba’s *Designing Social Inquiry* (King *et al.*, 1994). Critical realism, also known as scientific realism, is associated with the writings of Roy Bhaskar (Bhaskar, 1975) and shares with neopositivism a belief in causality, empirical testing, and a reality independent of the observer. However, critical realism emphasizes the importance of ontology (how researchers conceptualize what they study) over epistemology (how researchers know what they know) (Jackson, 2011: 26). The nature of the subject matter dictates the choice of methods, not the other way around (Patomäki and Wight, 2000b). In the social world, the existence of human agency and ability of social actors to alter their behavior in the light of social knowledge requires us to understand regularities and law-like features of the social world in a fundamentally different way from how we would understand natural regularities and laws. These questions of ontology shape the character of the account of science that critical realism puts forward.

Neopositivism and critical realism both presuppose “mind-world dualism.” By contrast, analyticism and reflexivity share a commitment to what Jackson terms “mind-world monism.” Mind-world dualism “maintains a separation between researcher and the world,” whereas the latter “maintains that the researcher is part of the world in such a way that speaking of ‘the world’ as divorced from the activities of making sense of the world is literally nonsensical” (Jackson, 2011: 35–36). Analyticism includes soft constructivist approaches to IR in the tradition of Weber. By contrast, for reflexivist scholars

the tools of knowledge-production are turned back on the situation of the scientist herself or himself; this reflexivity grounds or warrants empirical claims by relating them neither to a mind-independent world, nor to a set of cultural values, but to practices of knowledge production themselves.

(Jackson, 2011)

Hard constructivist approaches including Gramscian Marxism, post-structuralism, postcolonial, and much of feminist theory are reflexivist in orientation. A good example for the last approach is Chapter 9 by Sjöberg in this volume.

Overall, Jackson notes, IR uses a wide variety of different conceptions of science. Attempts to equate science crudely with neopositivism are grossly misleading. IR should define science broadly rather than narrowly, as neopositivist hegemony has wrongly insisted. Whether they realize it or not, IR scholars operate in an intrinsically pluralistic environment. They must recognize the

mutually exclusive nature of the “wagers” on which different conceptions of social science are based, and understand and appreciate the strengths and limitations of their own epistemological assumptions. This greatly complicates the possibility of achieving scientific progress within IR. Jackson’s book has made a strong impact by reclaiming “science” for the non-positivist community and providing a language for thinking about the philosophy of science that helps IR scholars become more self-conscious of the philosophical traditions within which they are working.

A little over a decade ago two other major texts emerged as key points of reference: Patrick James’s *International Relations and Scientific Progress* (James, 2002b) and Colin and Miriam Elman’s *Progress in International Relations Theory* (Elman and Elman, 2003). These contributions both fit firmly in the positivist (to Jackson, neopositivist) camp. Both focused on Imre Lakatos’s MSRP and its relevance for evaluating and assessing research in IR (Lakatos, 1970). James provided a definitive assessment of the evolution of structural realist theory by refining and elaborating Lakatos’s notion of a research program using work by Larry Laudan (James, 2002b; Laudan, 1977). By contrast, the Elmans collected a range of scholars to evaluate the major research programs in the field using the Lakatosian metric.

Due to its salience in the field, it is worth summarizing the core precepts of Lakatos’s MSRP. According to Lakatos, testing theories and falsifying them is only the first stage in scientific inquiry. Over time, theories evolve in the light of the evidence to produce a chain of successive iterations of development displaying expanding explanatory power. This chain of theories cumulates from a shared set of axiomatic assumptions and forms what Lakatos (1970) terms a “research program.” Research programs are comprised of three elements. The first element, the “hard core,” sets out a generic set of assumptions that all theorists within a research program share. This ensures that different theories in the same research program remain consistent with one another. The second element, the “negative heuristic,” defines the scope of the research program, setting out what evidence might be used to falsify claims. The third element, the “positive heuristic,” shows how the explanatory content of the research program is increased while remaining compatible with the hard core and negative heuristic. This framework offers a way around the problems of Popper’s falsificationism while avoiding the perils associated with Kuhn’s relativism. Theories are accepted or rejected not on the basis of an individual test, but in relation to the trajectory developed by a succession of theories that together comprise a research program. This approach provides structure and focus for scientific research, encouraging it to develop in a logical and cumulative fashion (Harrison, 2003, 2010).

James draws upon Lakatos’s ideas to set out structural realism as a “scientific research enterprise.” This notion synthesizes elements from Lakatos’s methodology of scientific research programs with Larry Laudan’s (1977) notion of a research tradition. James identifies six axiomatic assumptions as defining structural realism’s hard core and negative heuristic. The positive heuristic generated

by structural realist theory is demonstrated by disaggregating structure into a continuum of capabilities-based elements. This codifies an extensive body of realist research that previously lacked coherence. By elaborating the structural realist approach, James demonstrates the potential for systemic theory to provide a stable foundation for achieving scientific progress in the field. His book has established itself as one of the most important statements of structural realist theory.

Like James, the Elmans draw inspiration from Lakatos. In Part 1 of their volume, prominent experts apply Lakatosian criteria to chart progress within their respective research programs. In Part 2, they offer a range of commentaries on the strengths and weaknesses of Lakatosian methodology. The Elmans offer a systematic survey of cutting-edge techniques in terms of positivist metrics for evaluating scientific progress in IR and their study remains a critical point of reference. Overall, James and the Elmans together took our understanding and application of Lakatosian methodology and epistemological debates about the application of philosophy of science to IR more generally to a new level and in so doing paved the way for the current debates connecting to Chernoff and Jackson yet ranging beyond both.

All of these contributions have energized debates about the relevance and use of philosophy of science in IR. Previously implicit debates have become clearer, more explicit, and more informed in their theoretical sophistication. We also have moved beyond setting up debate simply in terms of the conflict between positivist and non-positivist conceptions of science. One of Jackson's main contributions is the identification of a four-party debate, with non-positivist conceptions of science offering a variety of alternatives for researchers to choose from. Disagreements between analyticists and reflexivists, for example, often are as or even more contentious than debates between neopositivists and critical realists. Equally, neopositivist accounts are riven with disagreement, ambiguity, and even self-doubt. Jackson's argument that neopositivists themselves cannot agree on much in terms of their definitions of science and criteria for measuring progress seems to be affirmed by the different metrics put forward by philosophers such as Popper, Duhem, and Lakatos, as well as the difficulties of applying these in a supposedly "objective" fashion to evaluating progress. Chernoff himself recognizes that, by his own criteria, only one research program in IR (the democratic peace) can truly be said to have progressed. Does one progressive research program make IR a scientific discipline? Perhaps not.

Yet, of course, there are also vociferous debates raging between broadly positivist and non-positivist approaches to science. All of these debates now are taking place in an atmosphere that is ripe for the present volume. In the wake of Chernoff's and Jackson's influential books, it is possible to have a mature and informed debate about different criteria for defining science and measuring progress in IR research. There is the opportunity to do so in a fashion informed by previous debates and also to use previous studies as a springboard to bring these approaches to a broader audience of substantive researchers. In the spirit of the sociable pluralist approach proposed earlier, this is precisely what the present

volume seeks to do. We may never truly resolve these perennial debates and questions, but we will have succeeded in our goal if we raise awareness and understanding of the issues at stake, and encourage scholars to think more deeply, critically, and explicitly about the philosophical issues they often have skated over too easily in the past.

Overview of the book's contents

This volume provides a survey of the state of the art in the philosophy of science and IR. Each of the editors and contributors holds different opinions and perspectives, we grind no axes, and make no a priori assumptions about there being a single accepted definition of science or set of metrics for evaluating progress. Following from that point of departure, the book is organized into two parts. Part I considers critiques of positivism/neopositivism in IR, from a variety of different perspectives, as well as an account of the historical evolution of the discipline. Part II focuses on the research program most commonly touted as being progressive in positivist terms, debating a variety of different perspectives on this claim about the democratic peace from *both* positivist and non-positivist angles. The volume weaves a rich tapestry of ideas from which readers can draw their own conclusions and reflect on their own approaches.

Given the centrality of *The Conduct of Inquiry in IR* to the present volume, it is appropriate that Patrick Jackson himself kicks off Part I. Jackson, in Chapter 1, offers a robust extension of his argument through a critique of the “lock” that neopositivism wrongly has on definitions of science in IR. Neopositivism is not actually more “scientific” than analyticism, critical realism, or reflexivity. However, what Jackson terms “the bias of science ‘tilts the field’ in a way that neopositivists can take advantage of their rhetorical position.” The result of this, he surmises, has been “to foreclose the possibility of non-Scientific IR for decades ... thus stacking the deck in favor of neopositivism” as the dominant epistemology.

Colin Wight justifies the importance of debates in the philosophy of science in IR and sets out his own scientific realist account in Chapter 2. He considers how scientific realism approaches the validity of social scientific work by envisaging theories as “maps” that guide social researchers through the complex ontological terrain they navigate.

Torbjørn Knutsen puts together his previous work in the history of IR (Knutsen, 2015) and the philosophy of science (Moses and Knutsen, 1998) to examine in Chapter 3 the evolution of IR as an academic discipline after World War I. Many “new discoveries” rehash earlier debates, arguments, and empirical claims. Moreover, the pattern of evolution for IR as a discipline shows that major events drive theoretical and empirical fashions. Scholarship follows the real world rather than predicting it as neopositivist lore might have us believe.

In Chapter 4, Annette Freyberg-Inan complements the discussion in the previous chapters with a specific focus on the role of theory for knowledge creation. She pleads for a view of the role of theory that is compatible with this

volume's guiding notion of sociable pluralism, while aligning most closely with Colin Wight's critical realist take on the requirements for theoretical validity and utility. The chapter also serves as a link between the two halves of the volume by drawing together arguments from all other chapters to facilitate discussion on the role of theory between and among the different takes on philosophy of science in IR represented in this book.

Fred Chernoff opens Part II in Chapter 5 with a robust defense of his argument about scientific progress in IR, notably with regard to the democratic peace. None of the contributors in Part I of the volume lead him to concede ground to his critics. Chernoff comes out unapologetically for the relevance of broadly positivist techniques as a means of theory evaluation even while maintaining an attitude of methodological pluralism.

Hayes and James, in Chapter 6, draw on the influential recent work of Sil and Katzenstein on "analytic eclecticism" (Sil and Katzenstein, 2010). By combining the democratic peace claim with systematism, Hayes and James arrive at similar conclusions to Harrison (i.e., as described momentarily) about fruitful new pastures to explore in democratic peace inquiry. However, they reach this conclusion via a very different route, grounding it not in Lakatosian methodology but in terms that are more influenced by the pragmatist tradition in the philosophy of social science. Remarkably, given James's previous work, this argument in favor of pragmatism and weak foundations shares affinities with Patrick Jackson's version of pragmatism in *Philosophy and Inquiry*, which is inspired by Dewey.

Harrison, in Chapter 7, critiques Chernoff's account of the democratic peace quite directly. However, unlike some other contributors, he does so from a positivist rather than a non-positivist perspective. Eschewing Chernoff's and Duhem's conventionalist methodology in favor of a Lakatosian approach, Harrison argues that dyadic democratic peace research has become degenerative rather than progressive. In order to stimulate progress, Lakatosian methodology guides researchers away from the dyadic approach that Chernoff argues for and toward a system-level mode of inquiry.

Ish-Shalom and Sjoberg offer – in Chapters 8 and 9 – to use Jackson's terms – analyticist and reflexivist approaches to evaluating the democratic peace research program. Ish-Shalom emphasizes the importance and centrality of norms and normativity in accounting for the democratic peace. Drawing on an increasingly sophisticated body of feminist IR theory, Sjoberg goes further insisting that the supposed "fact" of the democratic peace is really an ideological mask for a series of silences that reinforce hegemonies and perpetuate various forms of exploitation and violence in international politics.

We conclude this introduction by coming full circle to the epigraph from Fred Chernoff's *Explanation and Progress in Security Studies*. The quotation indicates that researchers in IR cannot be expected to devote their studies to debates about philosophy of science. However, they need to know enough about these debates to advance their own arguments within an intellectually coherent framework. This statement is consistent with a positivist philosophy of science position. Yet it is also consistent with the spirit of Patrick Jackson's *The Conduct of*

Inquiry in IR, which as we have noted offers a different account of science and scientific progress to that presented by Chernoff. Moreover, it is an observation that we as editors of this volume also would endorse. As a result of debates in the theoretical literature, IR is at last ready to take debates about epistemological questions forward in a truly meaningful way. This development is long overdue. It is the goal of our book to catalyze this process of collective learning in scholarship. That the debate is now beginning in earnest is perhaps finally a sign of *real* progress in IR!

Part I

Judging progress in the study of international relations



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1 The bias of “science”

On the intellectual appeal of neopositivism

Patrick Thaddeus Jackson

In *The Conduct of Inquiry in International Relations* (2011), I suggested that science is a diverse collection of ways of knowing, each of which have their own type of internal validity, and as such there is no warrant for dismissing any of the methodological positions that I outline in the book (neopositivism, critical realism, analyticism, and reflexivism) as inherently “non-scientific.” I am all for disagreements – “contentious conversations” serves as something of a slogan for what I hope the field will be increasingly characterized by in the future – but let’s disagree about *meaningful* things, like the similarities and differences between the results arrived at by wildly and significantly different approaches to the systematic production of worldly knowledge. Refusing to engage on the grounds that one’s interlocutor is not properly “scientific,” as long as all parties are actually trying to engage in the explanation of events and outcomes, is a philosophical non-starter.

The only definition of “science” that can do justice to this variety of ways of doing research in international studies is a broad one, more of a sensibility than an operational specification. The broad definition of science that I introduce¹ characterizes science as systematic, worldly inquiry subject to public criticism intended to improve its results. “Science” is precisely *not* an operational set of instructions, but is instead a characterization of that particular domain of human activity that aims at expanding a particular kind of human knowing: factual, explanatory, *impersonal*, and *detached* in a specific sense I will clarify. There is no such thing as “*the scientific method*,” although there are important differences between Science and other knowledge-domains such as Art, Ethics, and Engineering.² There is, accordingly, no philosophical justification for the equation of neopositivism with “science” that one finds explicitly or implicitly in so many contemporary discussions of method and methodology.³

That said, the sheer *volume* of methods books and articles that hum the neo-positivist tune versus the relative dearth of methods books and articles fleshing out alternatives is not irrelevant to my argument here. I suggest that this inordinate volume of attention devoted to the operational requirements of neopositivist (social) science is a puzzle in need of a solution, precisely because neopositivism has no exclusive philosophical lock on science per se. Although there are undoubtedly *sociological* reasons for the dominance of neopositivist (social)

science, reasons having to do with the nearness of neopositivism to the policy discourse of the important state funders of social-scientific research on and thinking about politics and international relations over the course of the nineteenth and twentieth centuries (Schmidt, 1998; Oren, 2002; Reisch, 2005), I suggest that these reasons are insufficient on their own – especially since they beg a question about the intellectual *appeal* of neopositivism. Yes, state agencies disproportionately funded neopositivist social science, but they were presumably acting on some sense that neopositivism was the way to go in social science, and it would be circular to explain that sense in terms of the effects of those funding efforts themselves. There is a difference between the sociology of knowledge and “the intellectual and epistemic constitution of fields, the premises that make it possible to speak about issues that matter” (Tickner and Wæver, 2009: 17–18), and I would go further and say that the former often *depends* on the latter, especially when we are talking about issues of methodology rather than issues of substantive theory. The “intellectual and epistemic constitution” of the study of international affairs as a neopositivist enterprise came first, and animated subsequent funding and hiring and publication patterns that filled in the operational gaps. And it is that *prior* appeal of neopositivism in the social-scientific imagination that concerns me here.

It is my contention that the appeal of neopositivism is made possible by the fact that neopositivism occupies a position in relation to my other scientific methodological categories that is similar – in a specific sense that I will define below – to the position that Science occupies with respect to other fields of human knowledge like Art, Ethics, and Engineering. Positional similarity does *not*, however, mean logical or philosophical equivalence, and conceptually speaking, Science as a domain contains far more than neopositivism, and “nomothetic generalization” does *not* exhaust Science. But the plausibility of drawing the line around Science at the borders of neopositivism stems from the historical evolution of the various boundary-demarkating gestures intended to separate Science from other things. Those gestures privilege a notion of knowledge as *episteme*, as factual, as devoid of perspective and thus constitutively general (Flyvbjerg, 2001). Neopositivism repeats those gestures *within* the domain of Science, thus claiming the role of “*really* epistemic knowledge” within a domain already constituted as epistemic with respect to non-Scientific modes of knowing. This fractal similarity (Abbott, 2001) lends advocates of neopositivism a rhetorical advantage in discussions about the character of social science, because they can draw on the broader discourse constituting Science, and wield this as a weapon against their opponents.⁴ It is thus “Science” – the rhetorical operation of boundary-demarkation around a style of inquiry, and the specific history of those boundary-demarkation gestures – that biases conversations in a neopositivist direction.

Science versus other modes of knowing

My daughter is in elementary school. At one point earlier this year an assignment prompt came home, telling her how to design her science project for the annual science fair:

A science project is a way to discover something. It is a test or experiment designed to investigate a particular question. To do a science project, you must have an idea about what you THINK will happen if you do something. You will have results that can be measured in some way. You will be able to show the results of your science project and tell whether or not your hypothesis was supported.

Just for fun, we could count up the philosophical holes and gaps in this account of science: “test” and “experiment” are made into synonyms; measurability applies to results but not to inputs; “if you do something” suggests a manipulability account of causation, which might raise some real problems for the very idea of *social* science (including, at least in the U.S., the strenuous objections of Institutional Review Boards to the idea that we have to *do* something to our subjects in order to conduct scientific research on them); and, perhaps most egregiously, the notion that a hypothesis can be “supported” as opposed to either being falsified or failing to be falsified.

Naturally, elementary school is not the place to look for philosophical consistency, but I will go out on a limb and suggest that this conceptual hybrid is what many, if not most, people think of as representing “*the scientific method*.” All the basic neopositivist elements are here: a hypothesis that can be tested against a reality that presumably exists independently of the tester; a preference for nomothetic explanation signaled in the tacit division of observed outcomes into a control and an experimental group; and the operationalization emphasis that neopositivism inherits from its logical positivist forbearers in the Vienna Circle. While it might be possible to track the circulation of such concise articulations as part of a causal argument about how this commonsensical notion of “*the scientific method*” gets produced and reproduced, my concern here is somewhat different. I will take it as a given that my daughter’s assignment looks to many, if not most, people like “science,” and ask instead: Why is this? Clearly it cannot be the laboratory-experimental aspects of the assignment prompt, because we use the word “science” to refer to things outside of a lab setting all the time (e.g., theoretical physics, the analysis of data collected from orbiting satellites or from telescopes, and the monitoring of ozone levels at the planetary poles). But there’s something there that says “science,” and I want to pull it out more explicitly.

Part of the reason for doing so is that what this brief prompt says about science is, I think, not all that different from what European Enlightenment thinkers have been saying about science in broad terms for centuries – even staunch anti-experimentalists like Thomas Hobbes. In Chapter 5 of *Leviathan* (1651), which all too few IR scholars read because it comes before the account of the state of nature that conventional IR erroneously thinks of as Hobbes’s main intellectual contribution, Hobbes presents reason as

nothing but reckoning (that is, adding and subtracting) of the consequences of general names agreed upon for the marking and signifying of our

thoughts. . . . In what matter so ever there is place for addition and subtraction, there also is place for reason; and where these have no place, there reason has nothing at all to do.

The consequences in question are causal consequences, expressed in logical form:

whereas sense and memory are but knowledge of fact, which is a thing past and irrevocable, science is the knowledge of consequences, and dependence of one fact upon another; by which, out of that we can presently do, we know how to do something else when we will, or the like, another time: because when we see how anything comes about, upon what causes, and by what manner; when the like causes come into our power, we see how to make it produce the like effects.

Although Hobbes famously opposed the idea that it was possible to determine facts and their relationship by experimentation (Shapin and Schaffer, 1985), the goal of his notion of science does not deviate in essentials⁵ from that expressed in my daughter's science fair prompt. Common to both statements is the notion that what makes a claim valid is something that is not reducible to an individual or personal whim; some kind of operative procedure that can be utilized by different people, be it an experimental or a logical procedure, is required in order to evaluate the claim and determine whether it stands. That procedure is *public* in the sense that it does not simply have to convince the investigator, but also has to convince an audience (actual or implied) capable of following the procedure and making its own determination about whether the chain of reasoning has proceeded properly. The warrant for a claim made in the domain of science is its systematic demonstrability before the relevant public, and this differentiates science from other domains.

There is one more element here that is critical to the definition of science, although it is somewhat harder to see because of a general shift in scientific ontology over the past several centuries away from explanations using divine or "otherworldly" factors toward explanations that confine themselves to "worldly" factors. Lebow (2003) comments that it is unusual and somewhat uncomfortable to open an IR book with a short story condemning Nixon to Hell, although this would have been the norm for a biographical explanation written centuries ago: the final judgment and end of life would have been an important part of a complete account of someone's life. But the general trend in scientific explanatory protocols has been away from such notions as the divine will, toward more "materialist" or "physicalist" categories – to the point where Searle (1995) can simply declare that for us the two indispensable background scientific accounts of things are *purely* material accounts (evolution and subatomic physics), echoing Weber's (1994) more expansive observation that the world has become "disenchanted." In Hobbes, as in my daughter's science fair, this worldliness manifests as a call for exact definitions and measureable categories, a call that excludes – implicitly or explicitly – references to magical or divine agency.

So the broad definition of science as a domain of human activity contains three components: systematicity, publicity, and (this-)worldliness. I find it quite difficult to conceptualize science outside of those rather expansive boundaries, because without all three of those components there would be no way for a claim to be warranted in a sufficiently epistemic manner. And that epistemic character, I submit, is actually what the call for science is all about: what distinguishes science is that its claims are warranted by impersonal reason and detached procedures, rather than being inextricably linked to particular sets of perceptions or values or substantive commitments (Rescher, 1997). Descartes’ declared goal in his *Meditations on First Philosophy* was to provide a rational grounding for the truths of the Christian religion that did not depend on prior acceptance of the authority of Christian scripture – in other words, to provide a more general warrant for those claims. We could trace this privileging of episteme down through the centuries, with stops at Locke’s condemnation of “enthusiasm” as a way of testifying the truth of a divine revelation (and his declaration that only “noxious creatures” who had “quit the principles of human nature” would not acknowledge the plain and rational law of nature), Hume’s stance that examining situations closely enough to evoke sympathy would lead us to notions of justice that everyone would agree with, and Kant’s notion that only in pure reason could we find sufficiently compelling principles to command the assent of all rational creatures.⁶

Epistemic knowing is also implicated in Dewey and Bentley’s (1949: 282) definition of science as “that particular form of *practical* human activity which is concerned with the advancement of *knowing* apart from concern with *other* practical affairs,” as only an impersonal, detached knowing could deliver the “liberative effect of abstraction” that frees a worldly claim from its particular circumstances and makes it available for application elsewhere. A personal science, a science that depended on sharing an individual investigator’s personal point of view, would be rather nonsensical, and the claims advanced in such a manner would forfeit the claim to epistemic distinctiveness on which we rely when advancing an explanation in the first place.⁷ Without impersonality and detachment, why should anyone pay attention to our claims unless they suit their prejudices or interests?

Two caveats. First, the claim to impersonal detachment that animates my definition is *relative* and *local* rather than *absolute* or *global*. There is no such thing as absolutely epistemic knowing or categorical systematicity/publicity/(this-)worldliness, but instead the call for moving along these dimensions is a call that works to differentiate science from other domains of human activity – and this differentiation is always indexical, always embedded in ongoing streams of action, and never definitively finished (just like the distinctions in Abbott, 2001). No fixed and firm country of science emerges from this process of ongoing differentiation and distinction; what we get instead are sets of provisional boundaries drawn between science and other domains of human knowing, in which the call for science comes out differently depending on the circumstances of its articulation. In Hobbes, the target against which science is deployed is religious

and scholastic authority; in my daughter's science fair, it is the unexamined hunch that needs to be distinguished from properly scientific explanation. But there is no promised land of "really real science" to be reached, and no sense in which we have to *accept* the claims of science to be purely epistemic knowledge to be valid claims.⁸ There is instead the ever-finer-grained fractal replication of the call for impersonal detachment in warranting claims contributing to human knowing.

Second, and as importantly, the definition of science I have extracted here basically never shows up in print or in practice as such, but is instead generally in evidence with the specific local coloration appropriate to one or another self-proclaimed scientific methodology. Hence for a neopositivist, science comes out as a commitment to hypothesis-testing for the purpose of refining nomothetic generalizations, whereas for a reflexive theorist science comes out as a commitment to the dialectical disclosure of transfactual structures that shape experience – and although both methodological standpoints contain a notion of "progress," they are not the same kind of progress, in part because each methodology cashes out systematicity, publicity, and (this-)worldliness differently. So the broad definition is synthetic, abstract, and less descriptive than it is the result of an effort to grasp, in idealized form, the *intention* of various efforts to (tacitly) define science by laying down rules for proceeding in a scientific manner – an intent which I would argue is, via systematicity, publicity, and (this-)worldliness, ultimately about generating impersonal, factual knowledge.

It would therefore be most accurate to say that my "grammatical" analysis⁹ seeks to capture not the practical politics of differentiating between scientific and non-scientific ways of knowing, but the conceptual conditions of possibility for such boundary work. When an organization or an individual seeks to define itself as "scientific," the conceptual raw material on which it can draw is shot through with this commitment to impersonality and detachment, specifically teased out in systematicity, publicity, and worldliness. While the *motivation* for doing so in any particular situation might have much to do with securing access to resources or achieving victory in some political struggle, the *effect* of drawing a boundary around science by distinguishing between scientific and non-scientific ways of knowing is to (re)produce Science as a distinct way of knowing the world. I add the capital "S" at this point to underscore the fact that the envisioned domain has something of a Platonic quality about it, although the analytical order I am advancing here is the reverse of Plato's: Science, as an idealized notion, emerges from efforts to maintain the distinction between scientific and non-scientific knowledge practices. This allows me both to maintain the distinctiveness of Science as an idealized way of knowing *and* to deny that there is any one scientific way of doing things; what there is instead is a family resemblance among boundary demarcations, which gives rise to the appearance of a stable something called Science.¹⁰

Positional similarity

The fractal repetition of these calls for impersonality also provides the key, I would say, to the intellectual appeal of neopositivism *within* the domain of Science. Just as Science distinguishes itself from other domains of human activity in terms of the impersonality and detachment of the epistemic knowledge that Scientific activity generates, neopositivism distinguishes itself from other systematic, public, and worldly approaches to the production of factual knowledge by claiming to be even more authentically impersonal and detached than those alternatives are. Neopositivism thus enjoys what we might call “the advantages of consistency”: everyone in the Science camp claims to be epistemic *relative to* other domains of human action, and neopositivists claim to be the purest instantiation of that stance. As above, so below, and neopositivists thus are able to deploy gestures bounding Science against other approaches to knowledge-production *that arguably belong within the Science camp* inasmuch as they also are intending to produce systematic, public, worldly knowledge.

This can be visually clarified by taking the definition of Science that I have proposed and dividing its root impersonality and detachment into two dimensions: *expressiveness*, the extent to which a claim expresses a value-commitment held by the claimant versus the extent to which a claim suspends those expressions of value in favor of focusing on the things themselves; and *evaluation*, the extent to which a claim passes judgment on a state of affairs versus the extent to which a claim states how things stand. Impersonality is a kind of non-expressiveness, to be juxtaposed to an explicit embrace of a value-laden perspective; detachment is a kind of non-evaluation, to be contrasted with an engaged way of knowing and being-towards the world under investigation. Cashing out things in this way makes possible the following 2 × 2 chart (Table 1.1).

Science occupies the upper left-hand quadrant here because it is claimed to be relatively impersonal and detached *compared to other domains* – which means that the call for Science is intended to distinguish a scientific claim from an artistic, ethical, or engineering claim. Science is the domain of knowing-that, of facts, of being able to rest secure on knowledge that is held to be independent of perspective and thus true. Science also gets firm lines around it because it is really mainly Science that tries hard to differentiate itself from other domains; the other boundary lines between domains are a lot more fluid and flexible.

Table 1.1 Expressiveness and evaluation

		Evaluation	
		Detached	Engaged
Expressiveness	Impersonal	Science (epistemic)	Engineering (technical)
	Value-expressive	Art (aesthetic)	Ethics (normative)

To canvas the other parts of the typology:

- Art I place in the lower left-hand quadrant because artistic statements are clearly expressive, but not usually evaluative (I am thinking here of the kind of philosophy of art associated with someone like Wassily Kandinsky in his writings on abstraction, e.g., Kandinsky, 1977). We hang things in museums primarily because of their craft in expressing something, not because of any judgment that they pass.
- Ethics occupies the lower right-hand quadrant because an ethical claim both expresses a value-commitment and uses that value-commitment to render judgment on what it seeks to depict. To treat something as an ethical claim, or to speak of ethical knowledge, is to call attention to the ways that the world is evaluated in and through the claim in question.
- Engineering occupies the upper right-hand quadrant, because an engineering claim is intimately wrapped up with the production of particular effects, and hence constitutes both an evaluation of the situation in terms of its as-yet-unrealized possibilities and a non-expressive depiction of those possibilities that can help ensure that they are actual potentialities instead of merely desirable consequences.

Another advantage of this depiction is that each of the quadrants other than Science shows a dimension on which that domain is less impersonal and detached than Science is, despite contributing to a distinct way of knowing.¹¹ Art, for instance, while highly systematic and worldly, is not subject to public criticism designed to improve it in the same way that Science is; the notion of “progress” in art is just not as well defined and not as broadly acceptable to practitioners. Ethics, while systematic and subject to public criticism intended to produce better ethical claims, is by definition¹² otherworldly, because it brings to bear standards that have at least one foot outside of the actual world – even if that is more a matter of the *form* of the claim than its *substance* – in order to judge that world. As for Engineering, systematicity is less important than efficacy, so even though the resulting solutions to practical problems are both worldly and subject to public criticism intended to improve them, we are here looking at a domain that is logically distinct from that of Science. All three of these domains are distinct from Science, albeit in different ways, and as such all serve different kinds of knowledge-interests: Engineering refines know-how, while Ethics helps us know where we fall short of our ideals and Art helps us know our value-commitments better and perhaps in so doing revise or refine them. And this fourfold in turn helps to sharpen the debate about *whether* IR should be purely a Science, or whether there is room in the IR field for Engineering, Ethics, and Art as well.¹³

It will not have escaped the reader that none of my categories mention “Politics.” That is deliberate. I have studiously avoided incorporating the term into the typology, because there are ways of thinking and practicing politics that inhabit each of these domains. A scientific politics would seek to provide

impersonal solutions to pressing problems; an engineering politics would look for workable solutions regardless of their systematicity and thus, perhaps, their procedural fairness; an artistic politics would look to establish and maintain value-commitments, and probably end up adopting something like the tragic sensibility found in Morgenthau and Weber; and an ethical politics ... here we run into Weber’s (2004) discussion of the challenges of an “ethics of ultimate ends,” and the thorny dilemmas associated with militant idealism and the role of religious institutions in civil life. Since politics is *what we study*, and what we study from a variety of angles, it makes little sense to confine ourselves to one *ex ante* definition of that object if we want to give ourselves the best chance of expanding our knowing about it.

Readers of *The Conduct of Inquiry in International Relations* may have noticed another oddity, which emerges from the ways that my synthetic definition of Science is intertwined with the portrayal of methodologies in IR in that book. Consider for a moment Table 1.2, drawn from the book.

Might it not be the case that this entire table *fits into the upper left-hand quadrant of Table 1.1* – and, further, that dualist/monist is the local, within-the-Science-quadrant, manifestation of the tension between value-expressive and impersonal claims, while phenomenal/transfactual is the local manifestation of the tension between engaged and detached claims?¹⁴ After all, critical realism and reflexivity are certainly more evaluative of the actually-existing world than either neopositivism or analyticism, and analyticism and reflexivity are more self-consciously expressive of value-commitments than either neopositivism or critical realism. The sort of fractal self-similarity that Andrew Abbott used to organize controversies over time within sociology may, therefore, prove useful in IR as well, if we can come to see methodological debates within IR-as-social-science as reflective, in relative terms, of broader distinctions relevant to the organization of human knowledge-producing activity in general.

The neopositivist’s view of the world

Besides linking intra-IR methodological controversies with broader currents of thought, though, the importance of the self-similarity I have presented here is that it helps us explain both the intellectual attractiveness of neopositivism and the “dialogue of the deaf” that ensues when neopositivists are confronted with work emanating from the other three boxes of the above typology. As before, the key here is two helpful mechanistic notions that Andrew Abbott (2001) introduces: the “advantages of consistency,” and the “New Yorker’s view of the

Table 1.2 Dichotomies

	<i>Phenomenal</i>	<i>Transfactual</i>
Dualist	Neopositivism	Critical realism
Monist	Analyticism	Reflexivity

world.” In a situation of fractal self-similarity, Abbott points out, each side of a distinction once made falls into further distinctions along the same lines, as when a left-right political split further issues in a relative left-right split emerging *within* each of the two resulting camps (an all-too-typical pattern in political life). This means that one component part of each of the second-round distinctions basically replicates the initial move that produced their whole camp: after the initial left-right split, the left-left and the right-right get to radicalize that initial split within their local settings. “The advantages of consistency” derive from the immediate credibility that such a position commands, as the left-left and the right-right can simply say that they are continuing the mission while the left-right and the right-left are placed on the defensive, easily accused of selling out and compromising.

Since these second-round distinctions replicate the initial distinction, the “New Yorker’s view of the world” kicks in: much as in the famous 1976 cover of the magazine *The New Yorker*,¹⁵ distinctions close to the viewer are shown in extreme detail, while everything further out is compressed into featureless masses. Abbott points out that in a situation of fractal self-similarity, the camps produced by second-round distinctions will tend to look, to those replicating the initial gesture of differentiation, not like distinctions within a single camp, but like the initial division into camps in the first place. So to the left-left, the left-right faction looks not like fellow leftists, but like rightists, *virtually indistinguishable* (at least for political and rhetorical purposes) from the entire “right” camp produced by the initial split. Needless to say this makes communication and compromise rather difficult, since the “consistent” parts of the second-round split perpetually conflate their local opponents with those in other camps, and are positionally tempted to regard themselves as the keepers of the flame against the apostates and heretics and other fellow travelers. The two mechanisms reinforce one another, creating dynamic pressure toward the extremes of a self-similar process of differentiation as moderate positions struggle to maintain themselves within the resulting field of force.

If we apply the same line of thinking to the call for detached, impersonal, epistemic knowing I have suggested forms the core of how we think about Science, we get the following depiction of how the world looks *from the point of view of a neopositivist* (Table 1.3).

Because the gesture differentiating Science from other domains is replicated by neopositivists *within* the sphere of Science, the illusion arises that other scientific methodologies *aren’t Science at all*, but are instead inhabitants of the separate domains against which the call to impersonality and detachment was

Table 1.3 The neopositivist world

	<i>Detached (phenomenalist)</i>	<i>Engaged (transfactual)</i>
Impersonal (dualist)	Neopositivism=Science	Critical realism=Engineering
Value-expressive (monist)	Analyticism=Art	Reflexivity=Ethics

originally directed. For the neopositivist, models thus become interesting pictures that have to be tested, causal powers are a messier gloss on cross-case covariation and basically indistinguishable from intervening variables, and critical dialectical engagements with the structures underlying experience look like normative critiques instead of having a distinctive worldly epistemic warrant. *Of course* neopositivists wouldn’t feel like it was worth taking those alternatives seriously, since *they aren’t Science*. Hence the odd notion that critical IR theoretical interventions can somehow benefit from the systematic study of cross-case regularities (Price, 2008). Hence the strange leap from “causal mechanism” to “intervening variable” (as in George and Bennett, 2005 and Mahoney, 2008, among others). Hence the manifest philosophical absurdity of the EITM project (Johnson, 1996; Clarke and Primo, 2007) and attempts to “falsify” Waltz’s account of the structure of the international system (Wæver, 2009; Goddard and Nexon, 2005). Hence also the (alarmist and misleading) fears of “relativism” voiced by neopositivists when confronted with the epistemic diversity of actually-existing IR in a social-scientific explanatory mode (Jackson, 2015a). *Of course* if one regarded oneself as the standard-bearer of impersonal reason, efforts to introduce and justify different and somewhat unfamiliar modes of epistemic knowing would seem not like an opportunity but like a crisis.

But the punch line here is not that neopositivism *actually* is more Scientific than analyticism, critical realism, or reflexivity. Self-similarity has another message: although the advantages of consistency and the New Yorker’s view of the world provide part of the dynamism that drives conceptual change, we should be wary of accepting the partial narrative spun from an extreme position as genuinely representative of either the state of play or of the whole field of possibility. This is especially the case when that extreme position is also the beneficiary of massive amounts of financial support and other elements of institutional power, which has certainly been the case for neopositivist IR ever since the field’s postwar reorganization around a search for “theory” that could function, in a U.S. context, to render the politically unpalatable exercise of raw coercive force on the global stage as a necessity rather than as a choice. The price of this political influence was the adoption of the discourse of “science” even by *realpolitik*-oriented intellectuals like Morgenthau (Guilhot, 2008); the result was to foreclose the possibility of non-Scientific IR for decades, and to lock in the gesture toward impersonality as a part of the field’s sensibility about knowing – thus stacking the deck in favor of neopositivism from the outset. Hence the “heroic narrative” (Ashley, 1984) of the development of IR knowing as the progress of Science, and the virtual disappearance over time of whatever skeptical and critical impulses were present among the field’s first scholarly generation.

All is, however, not lost for non-neopositivist Scientific methodologies. There may be a pervasive drag toward the upper left-hand corner of the typology, but this is not a categorical demarcation as much as an ongoing controversy. If step one is recognizing that, and step two (which I have only gestured at here) is disentangling neopositivist confections of other Scientific methodologies with non-Scientific modes of knowing, step three is showing how the other three parts of

the Science typology are equally good candidates to meet the basic standard of epistemic knowing, divided into impersonal and detached components, that suffices to differentiate them from Art, Ethics, and Engineering. Not that there is anything *wrong* with those three non-Science alternatives, and not that we might not want to have a global IR field that contains those modes of knowing alongside Science, but it is important for the sake of clarity to delineate the strengths and liabilities and potential contributions of different kinds of knowing. Category confusion – as when a theological claim that belongs in the domain of Ethics is mistakenly treated as an impersonal, worldly claim of the sort appropriate to Science – is the enemy of clear thinking, and leads to a lot of shadowboxing between caricatures instead of actual engagement.

My argument has been largely hortatory; as such it should be evaluated not by its “correctness” since it is unclear to me just how we would go about doing that in the first place, but by its effect when inserted into the actual, practical stream of human activity that makes up the present-day state of the IR field.¹⁶ I do not claim to have isolated or identified the transcendent essence of Science, but I do claim to have isolated or identified a useful definition that can promote better IR scholarship. And I claim to have diagnosed a persistent problem: the bias of the “Science” gesture itself tilts the field in such a way that neopositivists can take advantage of their rhetorical position and claim to be the truest representatives of Scientific knowing. Because we cannot abandon the category Science given its (social, cultural, philosophical) power both in the field and in the wider world – it is impossible to make a credible demilitarization pact wherein everyone in the field agrees to lay down their claim to Science, because the temptation to pick up that rhetorical and discursive instrument and deploy it against one’s opponents is too strong – the only practicable solution is to *re-engineer* the notion of Science to make it difficult to use as a way of shutting down contentious conversations about world politics. Perhaps one way of doing so is to rethink the boundaries of the IR field so as to make more room for Ethics, Art, and Engineering – alongside a suitably pluralized and chastened (Levine, 2012) notion of Science.

Notes

- 1 But, crucially, do not attempt to *argue for*. “What *can* be shown *cannot* be said.” Wittgenstein (1961), *Tractatus* §4.1212 – although I would say that the issue is less that something *cannot* be said *at all*, but *cannot be said within a particular language-game*. There can be no (social-)scientific justification of a definition of (social) science, so I think it is better just not to try to produce one.
- 2 The capitalization of these terms is intended to indicate and symbolize that when I use them I am referring to a domain of human activity, and not to any specific instance of anything that occurs within that domain. In this way, by Science I mean something like “the being-scientific of scientific activity,” that which makes scientific activity “scientific” in a classificatory sense. As far as I am concerned, this is a matter of broad sensibilities and family resemblances rather than some precise logical or Platonic form. See below.
- 3 Even when they claim not to, e.g., (George and Bennett, 2005; Mahoney, 2008;

Gerring, 2012). The whole chimera of “quantitative” versus “qualitative” research, which I critique elsewhere (Jackson, 2011: 67–69), only serves to distract scholars from the extreme *narrowness* of the issues supposedly at stake. (See Chatterjee, 2009).

- 4 Similarities to the argument about rhetoric and legitimation in Jackson (2006) and Krebs and Jackson (2007) are entirely deliberate.
- 5 Unless one thinks that the difference between the necessarily tentative results produced by an experiment and the “certain and infallible . . . signs of science” that arise when an investigator grounds his claims on the logical combination of appropriate general names is an essential difference. It is not, despite Hobbes’s declaration (1651: Chapter 5) that a general assertion has no conceivable possibility unless the assertion is true:

And therefore if a man should talk to me of a round quadrangle; or accidents of bread in cheese; or immaterial substances; or of a free subject; a free will; or any free but free from being hindered by opposition; I should not say he were in an error, but that his words were without meaning; that is to say, absurd

– and despite the enormous amount of ink spilled over the ensuing centuries trying to reconcile logical derivations and empirical observations. Both the rationalist and the empiricist sides of this controversy are aiming at the same thing, namely an *impersonal* warrant for worldly claims; next to that, the other parts of their disagreement appear as differences of tactics rather than strategy.

- 6 For an even broader canvas than the narrowly philosophical one I have hinted at here, which arrives at a not incompatible place, see Daston and Galison (2007).
- 7 The “conventionality” of scientific knowing, à la Duhem or Kuhn or Michael Polanyi, is still impersonal in this sense, because the resulting knowledge-claims are considered binding not just for one investigator but for the scientific community – a point made in IR discussions with great exactitude by Chernoff (e.g., 2005, 2009a).
- 8 Here, I would again contrast the *philosophy* and the *sociology* of science. From a sociological point of view, there is no such thing as truly impersonal knowledge, because “theory is always for someone and for some purpose” (in the oft-repeated Cox, 1996 formulation). But that doesn’t alter the fact that the *form* or *grammar* of a scientific claim inclines toward impersonality, such that “the speed of light is a scientific fact *for me*” doesn’t really even make sense.
- 9 In the Wittgensteinian sense (Wittgenstein, 1953).
- 10 Contrast my approach with that of Sandra Harding (Harding, 1998), who recommends *broadening* the notion of science so that it includes knowledge-practices that have typically been on the outside of the science/non-science boundary. This enables her to include “traditional” and “local” knowledges under science. But the costs of doing this are that we lose sight of precisely what gives the science/non-science distinction its immense intellectual appeal: the dream or vision of Science understood as impersonal knowing.
- 11 Yes, this is a Science-centric way of characterizing things. But I am attempting an intervention into and an internal critique of a field dominated by Science, not one dominated by Art or Ethics or Engineering.
- 12 Yes, *by definition*. “Natural law” doesn’t mean the same thing as “empirical generalization,” and lurking not far from the heart of any putatively “naturalistic” ethics is a value-commitment that cannot itself be reduced to a set of worldly facts. This is old news in IR, or should be, since Arnold Wolfers pointed it out decades ago (Wolfers, 1952; his essay on “Statecraft and Moral Choice” is also highly germane here). Yet another casualty of the “descent of political theory” (Gunnell, 1993) is that we often fail to recognize an ethical claim when we see one, and we imagine that there are empirical resolutions to ethical controversies.
- 13 I address that question more directly in Jackson (2015b).

- 14 Hidemi Suganami's (2013) and Torsten Michel's (2013) suggestions in the *Millennium* forum on *The Conduct of Inquiry* that dualism/monism is actually something like representation/expressivity is an important part of my thinking on this point.
- 15 http://en.wikipedia.org/wiki/View_of_the_World_from_9th_Avenue.
- 16 If, and only if, there were a single and sharply defined Scientific methodology would it be possible to evaluate my synthetic definition in terms of its correctness – and parenthetically, if one actually believes in such a single sharp definition, my definition will appear incomplete. This is *deliberate*. I suspect that many of our difficulties in the field when it comes to engaging one another's work stem from the insistence that Science is a methodology rather than a domain of human activity. My particular synthetic definition is thus offered as a practical corrective rather than a philosophically superior stance.

2 Maps, models, and theories

A scientific realist approach to validity

Colin Wight

Introduction

Matters of fact are only very partial and, I would argue, very polemical, very political renderings of matters of concern and only a subset of what could also be called states of affairs. It is this second empiricism, this return to the realist attitude, that I'd like to offer as the next task for the critically minded.

(Bruno Latour, 2004)

Nicholas Kristof's *New York Times* essay "Professors, We need You!" (Kristof, 2014) produced a storm of protest from political scientists keen to demonstrate how relevant they are. Kristof's argument was that some of the smartest thinkers were professors, but that they were missing from the great public debates. Kristof singled out political science as the discipline least relevant and blamed the discipline itself. Political science, he argued, was committing suicide in terms of practical impact. Turgid prose, increased specialization, and quantitative forms of analysis have rendered political science an "academic" subject in the derogatory sense. In short, political science did not matter; it was irrelevant to public debates. The same could be said of International Relations (IR), an overlapping interdisciplinary field with shared desires for policy relevance.

The criticism that followed did little to dispel the notion that he had put his finger on something. "We are engaged," the chorus sang, "we tweet, we write blogs, we talk to policy makers; we matter damn it" (Drezner, 2014).¹ Yet, the defensive nature of the responses seemed to confirm Kristof's diagnosis. Relevance became confused with contribution, and that Kristof's argument needed to be engaged only proved that he was right. Despite this, however, there are problems with Kristof's piece.

First, relevance is confronted by all the social sciences, not just political science. Social scientists have to bear some responsibility for this, and one could point to turgid prose, specialization, and quantification as playing a role, but also the relativism that infects the social sciences, as well as the fact that some within the social sciences reject their scientific status. After all, if all claims are relative, why should policymakers accept any claim other than their own, and if the

research is not scientific, why should they use or fund it? Second, while social scientists do bear some responsibility for the lack of relevance, policymakers and politicians can also be faulted for ignoring relevant research or looking for “off the shelf,” simplistic solutions to problems. Third, the relationship between the state and social science has changed. There is a close and well-documented relationship between the state and the social sciences (Bouwel, 2009; Carroll, 2006; Wagner, 1991). When the social sciences first emerged they were often seen to be handmaidens of the state; they were “state social sciences” (Holmwood, 2007). In the period following World War II, in particular, politicians turned to the social sciences for advice concerning social reconstruction. This changed, however, when Thatcher and Reagan rolled back the state; if the state is to be minimized then there is less need for social scientific knowledge.

It is, however, ironic that Kristof cites economics as one of the social sciences that has most relevance. It is ironic because commitment to formal models and quantification within economics runs counter to Kristof’s critique of such models in political science. Indeed, many of those committed to such models in political science have drawn inspiration from economics (Waltz, 1979). But also, given that economics failed to predict the Great Financial Crisis (GFC) of 2008, and was complicit in its production, economics hardly seems a good role model. Yet Kristof’s appeal to economics illuminates an unspoken, and widely accepted, assumption that grounds his critique. What sets economics apart is that, among all the social sciences, it is considered to be the most scientific.

The status of the social sciences is linked to the expectation of validity associated with science. International relations (IR) has always struggled to demonstrate its scientific credentials; this explains why debates from the philosophy of science have increasingly played a role in its theoretical debates. The concern to present itself as a science has been central to the field. E.H. Carr compared the science of his realism to the “alchemy” of idealist approaches; the implication being that realism was more valid because it was science. Morgenthau, despite criticizing the attempt to construct a science of IR in *Scientific Man and Power Politics* (Morgenthau, 1947), still declared IR to be governed by “objective laws” (Morgenthau, 1978: 4). Marxists were keen to portray their theory as a scientific account of the structural causes of exploitation within capitalism. The march toward science, rigor, and more systematic analyses became a standard trope of presidential addresses in both IR and political science. By the time Waltz’s *Theory of International Politics* was published, references to the philosophy of science had become commonplace. Citations to Kuhn, Popper, and Lakatos became de rigueur as competing approaches either claimed the scientific high ground or dismissed the scientific pretensions of alternatives. There is logic to this. Whoever defines science controls the field.

Beyond this, why should policymakers and publics struggling to make complex policy choices turn to academics if the research is not scientific? Paradoxically, although the natural sciences only matured when they left philosophy behind (Mason, 1953; Shapin, 1996), the social sciences are constantly turning to philosophy to legitimate their status. In some respects, it might be better if the

social sciences could do the same and simply get on with research unencumbered by the weight of validating themselves in terms of the philosophy of science. This would be my preferred solution, as when stripped of false and misleading philosophies of science the social sciences could follow the lead of the natural sciences and become relentlessly realist.

Indeed, as Ian Shapiro has noted in his *Flight from Reality in the Human Sciences* the lack of realist commitments has being most damaging to Economics, long considered to be the most scientific of all social disciplines (Shapiro, 2005). Although an attractive proposition, however, it is not one the social sciences can embrace. The route back to a “relentless realism” has to be argued for, and it is simply not possible to move there unless the arguments for it can be made and understood. Social science critics of philosophical realism have always worried that realism represents a dogmatic approach. But this is a category error. Realism and skepticism go hand in hand. Realism is not the dogmatic assertion of claims concerning what is real, but the incessant questioning of all claims. This applies to assertions concerning the power/knowledge nexus as it does to anything else. Claims about the power/knowledge nexus are still subject to the reality clause; is it really the case that power and knowledge are related in X way, or not? Hence, the question is not realism *or* power/knowledge, but realism *and* power/knowledge. Those who would insist on taking sides in this either/or dichotomy are being unrealistic; you cannot have one without the other. Which is why it is easy to point out the hidden realist assumptions in all approaches.

In order to demonstrate the necessity of this relentless realism I tackle a hard case for realism: the realism or otherwise of maps. Maps are clearly not neutral, realistic depictions of some realm. Yet maps also have embedded in them realistic assumptions and it is only when we understand these that we can explain why maps work. Moreover, in considering the relentless realism of maps we can also identify how theories might be validated. In short, the issue of the validation of theories also has to be realistic.

In section one, I defend the role of the philosophy of science in IR. Section two outlines my own account of scientific realism. Here, I do not try to persuade critics of the validity of scientific realism, but set out my own understanding of it to avoid confusion surrounding some key arguments and concepts. I concentrate my attention on how scientific realism deals with epistemology and the power/knowledge nexus, since on this issue it parts company with other existing approaches in IR. Sections three and four put this realism to work and explore one way scientific realism might approach the issue of validity in social scientific research through an exploration of theories as maps and models.

My answer might surprise. Although I remain committed to scientific realism, I suggest that properly conceived it is not (always) in opposition to alternative approaches. While science ultimately aspires toward realism, it does not always get there, and the “way points” on the journey are as valuable for their failures as they are for any successes they produce.

A fish out of water? Philosophy of science in IR

Debates about the scientific status of IR have reached an impasse (Wight *et al.*, 2013). Calls for analytic eclecticism (Sil and Katzenstein, 2010), theoretical pluralism (Jackson, 2011) and claims that “isms are evil” (Lake, 2011) are representative of a widely held view that debates surrounding the philosophy of science in IR are getting nowhere. According to Lake, the issues under debate have no solution and are distracting the discipline from understanding the practice of international relations (Lake, 2011). These arguments are not new. In *Orders of Discourse*, John Gunnell provides a coherent and sustained set of arguments against the deployment of the philosophy of science (Gunnell, 1998). According to Gunnell, the “production of knowledge, whether historical, analytical, or empirical, has fallen aside as the conversation of political theory has devolved into a churning of the ideas of a handful of contemporary authors and into a chase for new philosophical authorities” (Gunnell, 1998: xii). This bleak assessment motivates his attempt to put the philosophy of science in its place. Gunnell’s answer to this problem is similar to Homer Simpson’s, “Kids, you tried your best and you failed miserably. The lesson is, never try.”

Gunnell’s criticism is that social science has been impoverished by meta-theoretical images of the nature of inquiry and that this has contributed to a retreat from consideration of the relationship between political theory and politics (Gunnell, 1998: ix). Political theory has failed to confront the pragmatic dimension of the relationship between political theory and politics. To put it another way, Gunnell thinks that political scientists have sought meta-theoretical answers to essentially practical problems. I am not convinced these claims are correct.

First, while it is true that there has been an increase in meta-theoretical speculation in political inquiry since the mid-1980s, it is not the case that the majority of research in political science and IR has got involved in these debates. The debates are carried out by a specific group of scholars, but largely they are debating among themselves. It is not clear that the rest of the discipline are listening or care or that these debates are detracting from the development of substantive theoretical accounts of politics.

Second, none of the contributors to this debate, apart from Fred Chernoff (Chernoff, 2007a), who directly links his position to the ability to predict, argue that meta-theoretical issues can provide answers to practical problems. Certainly, there have been attempts to use frameworks drawn from the philosophy of science to legitimate particular “research programs” in the field, and Imre Lakatos’s sophisticated version of Popperian falsification seems to be the default position (Elman and Elman, 2003). But these approaches use Lakatos and others to assess particular approaches rather than assuming a direct link from the philosophy of science to substantive theory.

My concern, by contrast, always has been to reorientate the field away from a confused commitment to two problematic notions thought to be essential to the defense of “science” in IR. First is the attempt to arrive at some epistemological

a priori position that underpins all one's research (something that is difficult to achieve because of the confusion surrounding the use of this term); second is the search for appropriate scientific methods as defining what counts as good science. Contrary to these epistemological and methodological attempts to frame cross-theoretical debate, I consider the real dividing line among competing approaches is ontology. There is no epistemological basis to science; science, should, and does, use a range of epistemological supports (what I have elsewhere called epistemological opportunism; see Wight, 2006); and there is no such thing as the scientific method; each science has to develop its own methods appropriate to the object under study.

In short, we need to get the theoretical claims, understood as ontology, on the table before we can challenge those making the claims as to how they know (the epistemological issue) a particular claim is valid (understood in an approximate not absolute sense), or question the appropriateness of any chosen method. Yet, this is not the whole story, and there is a series of difficult issues about how the social is constituted, whether social wholes exist, whether non-observables should be treated realistically, instrumentally, or ignored, and so on. And because of these differences, we have debates that, rather than reinventing the wheel at every turn, draw on alternative, non-disciplinary, sources of support. After all, it would be a strange account of social action that did not draw on existing theories of social action, or theories of language use in IR that did not draw on established theories of language outside of the field.²

Yet, even if the discipline is keen to assert its scientific credentials, why does it need to turn to the philosophy of science in order to do this? After all, one aim of IR is to understand, and explain, politics. Surely, we can just get on with the task of producing substantive theories of our chosen realm, in the manner of the natural sciences and leave philosophy to the philosophers? Attractive as this might seem, there are historical, ontological, and ethical-political reasons why this is not possible, at least for the social sciences.

Historically, we cannot leave philosophy behind because the discipline already has well-defined philosophical narratives about what constitutes good science that inform its methods, theories, and discourses. To abandon philosophy, at this stage of disciplinary development, would be to leave in place a form of political analysis that is already deeply embedded within particular philosophical assumptions that are problematic. Moreover, the philosophies of science that dominate the discipline can only be tackled at the appropriate level – and that is at the level of philosophy. Like it or not, even if the idea of an IR without philosophy was attractive, it could only be achieved through more, not less, philosophy.

Another reason for maintaining a role for the philosophy of science and the philosophy of social science in the social sciences is the ontology of social practices (what they are and what they involve). Philosophy forms part of the object of study for the social sciences because the practices of the actors in that world are embedded within philosophical positions. Indeed, Peter Winch thought social science was nothing more than a form of philosophy (Winch, 1958). And by this,

I do not mean the manner in which certain political leaders are said to be guided in their practices by an explicit engagement with the philosophies of Plato, Machiavelli, and Locke. No, even a social actor with little, or no, awareness of political philosophy, metaphysics, the philosophy of social science, ethics, epistemology, or the whole range of philosophical discourses, can only act on the basis of some or other account of the world: What is truth? What is a fact? How do I know anything? How should I live? Who am I? Who are We? What is a cause? What is an explanation? These are basic philosophical questions that play a role in almost all social practices, even if those engaged in those practices rarely stop to reflect on such questions. Whether or not they are aware of it, everyone possesses and operates on the basis of at least an implicit sense of epistemology, ontology, and methodology.

In many respects, unreflective social practices are governed by answers without due consideration of the questions. Part of the rationale for university education is to think about the questions, and one important function of any properly conceived social science is uncovering just those assumptions that underpin social practice, particularly in those instances where the actors involved are not aware of the role and function of those philosophical assumptions (i.e., that the truth is self-evident, that to believe is to know, that social wholes do not exist, etc.). The objects of the natural sciences, on the other hand, have no such philosophical issues that govern their practices; or at least, as far as we can tell they do not.

At the ethical-political level, the need for reflection on social science also is clear. Social scientific results are not simply descriptive but also, potentially, normative. Given this property, and the unique place of science in the world, it is appropriate to examine the claims produced by the social sciences in terms of their claimed scientific status. The claims of the natural sciences also should be subject to both internal and external scrutiny, but this is much more pressing in the social sciences. The reasons are that the status of social science already is highly contested and potential implications in terms of social action are more immediately apparent. The claim that a certain gene should not reproduce is meaningless; how can a gene that does reproduce not do so? Yet to imply, for example, that single mothers and crime rates are related is an altogether different claim, and carries with it a normative element (it is also wrong). The social sciences study human beings, their practices, and their relationships, and most research into human practice will have normative implications for that practice. Natural sciences, on the other hand, can have an impact on human practices, but they do not necessarily involve a critical comment on those practices. The social sciences always, potentially, do.

Finally, much of the angst concerning the role of the philosophy of science in the social sciences concerns the way in which its deployment is said to be about the establishment of secure epistemological foundations. This is Gunnell's position, but also Kratochwil's (2003) and that of Monteiro and Ruby (2009). In fact, it is endemic to disciplinary understandings that these issues are primarily epistemological. My view is that epistemology plays a different, although important,

role in any properly conceived science, and understanding these debates in terms of a search for secure epistemological foundations is flawed. Philosophy has failed to provide any firm epistemological foundations, and the social sciences cannot put everything on hold until they produce them. Current wisdom in philosophy is that no firm epistemological foundations will be forthcoming. Which does not make epistemology unimportant, but it does mean any search for epistemological commitments is misguided.

Scientific realism: a sketch

The basics of scientific realism are well known, although not well understood. First, it is a philosophy of and for science. Scientific realism makes sense of what scientists do, and can be considered a “theory of scientific practice.” It provides the best explanation we have for the success of science. Indeed it is difficult to make sense of what the sciences do, and the success they have, unless we assume that scientists are realists, and that the theoretical accounts they produce have some purchase on the world. The search for dark matter makes no sense unless we assume that the scientists searching for it are attempting to ascertain if it exists. The protracted and expensive search for the Higgs boson particle makes no sense unless we assume scientists wanted to know if it exists (Carroll, 2012). If neither dark matter nor the Higgs boson could be found, the realism intrinsic to science is untouched; it is the search that validates realism, not the success.

What differentiates scientific realism from positivism and alternative accounts of science is the treatment of non-observables. And since much that we study in the social science is non-observable (beliefs, structures, emotions, intentions, and so on) this takes center stage. Nothing I can say will convince the critics of scientific realism, yet many of the criticisms are based on fundamental misunderstandings. Chernoff (2007a), for example, criticizes scientific realism for suggesting that social science research cannot proceed unless a theoretical ontology is developed in advance. Yet the position he adopts is consistent with scientific realism. How could theoretical posits be posited if not through theorizing? Theoretical posits are the proposing of an ontology.

Once a phenomenon of interest is identified then we need some theory that attempts to explain/describe those phenomena. Theory Y might explain it in terms of X, and theory A might explain it in terms of B. But we can only test the validity of either theory if we have some idea of what X and B are. We could not begin the search for dark matter unless we had some idea of what it was, or how we might detect it. Hence specification of the objects (ontology) must come prior to attempts to validate claims about the objects. You cannot find a needle in a haystack unless you have some idea of what a needle is. It is not clear how Chernoff thinks his position differs from scientific realism, but I have always insisted that my main concern is with the ontologies embedded in different theories (Wight, 2006). These ontologies can only come about through the act of theorizing; where else? By in “advance” scientific realism means “in advance” of concrete research to determine the validity of the claim concerning X.

Likewise, it is impossible to determine how data concerning a claim about X might be gathered unless one has some idea of what methods are appropriate to the object. This means that the chosen method has to be appropriate to the task at hand and specification of the object domain. If your chosen question demands quantitative analysis then that is the most appropriate method; if not, there is no point in using quantitative methods. Methods cannot precede the theoretical specification of ontology. Belief in the priority of method is an instance of Maslow's hammer: "I suppose it is tempting, if the only tool you have is a hammer, to treat everything as if it were a nail" (Maslow, 1966).

Another point of contention is whether the specified ontology should be understood realistically or instrumentally. Chernoff also takes issue with my realist take on this, claiming that I am wrong to suggest that, if instrumentalism is correct, science would not have progressed (Chernoff, 2009a). He criticizes my examples of the dispute between Galileo and the Catholic Church and Bohr's account of quantum theory. Now, it is a matter of fact that the Church did advocate an instrumentalist account of Galileo's theory. And my claim is that had this account been accepted then this would have held back science. That was the point of the Church's intervention. That they failed is due to the realism that drove Galileo and the scientific establishment. Chernoff can only argue that instrumentalism would have made no difference with the benefit of hindsight. Had the scientific establishment remained happy with the instrumentalist treatment of the new theory, there would have been no need to go beyond the calculations and find explanations for them. It is for exactly this reason that Galileo is claimed to have uttered *Eppur si muove*. Despite the insistence of the Church that the new theory be interpreted only as a calculating device, Galileo thought differently.

Likewise, if the scientific community had accepted Bohr and Heisenberg's instrumentalist view of quantum mechanics then there would be no search for dark matter or the Higgs boson. Quantum mechanics is often said to be the most empirically verified theory science has produced, next to gravity, and the calculations bear this out; they have enabled us to do all sorts of useful things with the theory without having a full understanding of it. But despite this, the scientific community wants to find out if the postulated entities exist or not. Scientists, such as Bohr and Heisenberg, can espouse instrumentalism; thankfully science practices realism.

An example comes from the causes of cholera. In 1854, London had experienced sporadic outbreaks of cholera. The dominant theory at the time was known as the miasma theory, which suggested that diseases such as cholera were caused by "bad air." The widely held miasma theory had been accepted from ancient times; there was a consensus about it. In August of 1854, there was a particularly severe outbreak of cholera in the Soho area of London, in which 127 people died. John Snow was a local doctor and skeptical of the miasma theory. Through charting of the pattern of the outbreak, he suspected that the source was a water pump in Broad Street. With permission, Snow removed the handle from the pump and incidences of the disease decreased. This led to wholesale changes in sanitation.

Still, although Snow had a successful theory of how to control cholera, he did not know what it was about the water that spread the disease. In consistent instrumentalist fashion, he knew how to control the spread of cholera, but he did not know why contaminated water led to cholera. According to instrumentalism all the necessary conditions for good science have been met. If we treat the water “as if” it spreads disease, and our treatment of it in that way allows us to exercise control, then we have advanced as far as we need. Indeed, we can even predict a decline in outbreaks of cholera if we clean the water; we can predict but not explain. Yet, this was not sufficient for science; years later a theory of the mechanism responsible for cholera was provided (Johnson, 2006).

This commitment to realism has been an intrinsic part of science and also should be part of the social scientific attitude. Critics view this realism in negative terms, supposing that it suggests a dogmatic commitment to the ontological posits in any given theory. Actually, the reverse is the case. Realism is related to skepticism and adopts a negative stance toward claims. The realist constantly wants to know if “this is really the case,” rather than dogmatically asserting that “this is the case.” Also, scientific commitment to realism does not lessen the fact that instrumentalism, pragmatism, or conventionalism also play a role in science. At certain times – quantum mechanics is a good example – we have to accept an instrumentalist treatment of theoretical posits. And conventionalism, particularly in terms of measurement, is a necessary aspect of any science.

However, although necessary, neither instrumentalism nor conventionalism is sufficient and at some point the scientific community will push beyond both toward a realistic account. What this does demonstrate, however, is how realism, contrary to many of the other approaches, is less limiting. Realism does not constitute a barrier to “utility” being an important aspect of the assessment of validity. Nor does it deny the importance of consensus and the need for conventions. It simply demands that these are not sufficient in terms of a scientific explanation. Instrumentalism and conventionalism on the other hand do want to place a block on the road toward realism. In doing so, they block the advance of science.

But where does this leave epistemology? Let me state it plainly: the way IR currently understands the issue of epistemology is confused, incorrect, and a barrier to constructive debate across differing theoretical perspectives. I refer here to the widespread view that positivism, post-positivism, rationalism, constructivism, feminism, and postmodernism, for example, are epistemological positions. This treats the debate between positivists and post-positivists as a debate between competing epistemologies. This is spectacularly wrong. Positivism is *not* an epistemology; it is a philosophy of science. Yet the way the discipline treats epistemology implies that everything produced by a given approach is knowledge; that we have knowledge produced by positivism, knowledge produced by post-structuralism, knowledge produced by feminism, and so on. Yet what these approaches produce are claims, and some of these claims may actually be doxastic (i.e., relating to beliefs) rather than epistemic. The epistemological issue is one of how to sift through these claims in order to determine their epistemic status. If a positivist claims X, we want to know what are the grounds

for that claim; we want to know what is the epistemological support. When confronted with the question “how do you know X?” the answer “because I’m a positivist” is not an epistemological response, it is a statement of identity.

Thus “epistemology, properly construed, is, I will maintain, a post-hoc enterprise contingent on substantive theory and scientific practice” (Gunnell, 1998: 7). A philosophy of science is not an epistemology. One reason why this account of epistemology has become accepted practice can be located in the manner in which positivism attempted to delineate legitimate knowledge (Kolakowski, 1968). Positivists claimed that only knowledge produced according to positivist principles could be knowledge. This explains why positivism came to be known as an epistemology. But we should not accept this limited account of epistemology.

None of this should imply that epistemology is unimportant. Epistemology is vital. Its value, however, is post hoc, and always in relation to specific claims; claims which are embedded with ontological considerations and/or derived from the application of particular methodological techniques. As such, a researcher has no chosen epistemological position prior to making a particular claim and the epistemological support advanced for any given claim will vary depending on the content of that claim. Epistemological debate in science, as opposed to philosophy, never operates in an ontological void.

So what is epistemology from a scientific realist perspective? It is the branch of philosophy concerned with the theory of knowledge. At the heart of epistemology is an attempt to chart the difference between belief and knowledge. The main problems with which epistemology is concerned include: the definition of knowledge and related concepts; the sources and criteria of knowledge; the kinds of knowledge possible and the degree to which each is certain; and, the relation between the one who knows and the object known. Epistemological questions are those concerned with the nature and derivation of knowledge, the scope of knowledge and the reliability of claims to knowledge. In the social sciences, the situation is even simpler; epistemological questions are concerned with the grounds we have for accepting or rejecting claims. Or, how can we demonstrate that a statement concerning X represents an epistemological claim rather than a belief, a guess, or a hunch?

All epistemology is ultimately rationalist. Even empiricism rests on the assumption that it is rational to believe that there are “experiences.” This allows us to see that rationalism (the use of logic and reason) stands at one end of a continuum, with empiricism (the use of the senses) at the other. Viewed this way we can see that it is possible to combine the extremities of epistemological debate: rationalism and empiricism. All epistemologies, then, with the exception of radical skepticism, which constitutes a denial of epistemology, are admixtures of rationalism and empiricism. Viewing epistemology as a continuum helps us to go beyond debates about the validity of forms of knowledge conceived of as an oppositional dualism between truth and untruth; as in the debate between scientific or metaphysical forms of knowledge; or rationalist versus empiricist explanations. These dichotomous ways of framing the issue, which surface in

the social sciences, do not do justice to the complexity of the issues involved. The tables from Jackson in Chapter 1, while valuable in stimulating consciousness about the issues, also serve as examples of dichotomous framing.

Moreover, epistemology refers to an act of human knowing. Humans only have a limited number of ways of knowing as a result of their cognitive faculties. This is what Hollis calls the “epistemological unity of mankind” (Hollis, 1979: 225–232). These ways of knowing should not be viewed as mutually exclusive; nor should we privilege any epistemological approach. We need to apply all our cognitive capacities, even more so in science. Thus we experience the bent stick in the water, but we do not assume that the stick is really bent in the water. Rationalism and empiricism are both required to arrive at an adequate account of this phenomenon. Equally, this interplay of rationalism and empiricism is not restricted to Western scientific discourses: antediluvian peoples reliant for their livelihood on fishing also developed a rational explanation for their experience of sticks bending in water. Nor should this be seen as a subsumption of empiricism under rationalism; rather, our experiences only make sense when rationally ordered. Thus as Feyerabend puts it:

Things have to be done in concrete circumstances and not according to a general recipe. I regard the philosophical position of relativism as silly because it assumes what never happens, namely no exchange. I also regard the philosophical position of objectivism to be silly. They are two sides of the same coin.

(Parascandalo and Höslo, 1995: 137)

Feyerabend is pointing out the futility of attempting to address epistemological questions in black and white terms, and in advance of ontological considerations; that is why he insists on the concrete circumstances. He is advocating that we should be epistemologically pragmatic and reject all attempts to outline an a priori account of what constitutes knowledge. This is consistent with Sjöberg’s argument from Chapter 9 in favor of pursuing IR independent of the concept of cumulation. Often in IR we find scholars wedded to what I have called the “foundational fallacy.” According to this dogma, if we cannot have absolute untarnished access to knowledge, there can be no knowledge. This position, as Feyerabend makes clear, is untenable. As William James argued, “when we give up the doctrine of objective certitude, we do not thereby give up the quest or hope of truth itself” (quoted in James and Murphy, 1995: 17).

According to Haack, what we do when addressing epistemological questions is something much less ambitious than hope to attain infallible knowledge, but something altogether more optimistic than deep skepticism. Epistemological justification is really a matter of being “more/less justified in believing” something (Haack, 1993: 2). All human knowledge is fallible, but this does not mean that all knowledge-claims are valid. Following Roderick Chisholm (1989: 16), I suggest that we conceive of an epistemic hierarchy:

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- 6 Certain
- 5 Obvious
- 4 Evident
- 3 Beyond Reasonable Doubt
- 2 Epistemically in the Clear
- 1 Probable
- 0 Counterbalanced
- 1 Probably false
- 2 In the Clear to Disbelieve
- 3 Reasonable to Disbelieve
- 4 Evidently False
- 5 Obviously False
- 6 Certainly False

Such an approach is not without its problems, not least because the meaning of all of the above “levels of knowledge” would be susceptible to interpretation. However, the epistemic hierarchy does allow us to reject static polarities such as “true” and “false.” As far as the actual practices of scientists are concerned, as opposed to philosophical descriptions of them, their activities lend support to the epistemological eclecticism advanced here. They operate with epistemological positions functioning as “rules of thumb” rather than all or nothing positions. The scientist begins by using one rule of thumb, but if it fails, she introduces another. Rules of thumb, argues Feyerabend, constitute a “toolbox.”

Never throw away the tool box, never declare the tool box itself to be the one right thing or one tool in it, but use it, extend it, disregard it sometimes, according to the case with which you are dealing, because you never know what you will run into.

(Parascandalo and Höslo, 1995: 123)

This demonstrates the importance of the linking of epistemology to ontology, as well as presenting scientists as epistemological opportunists whose practices bear little or no relation to the dogmatic accounts produced by philosophers of science, or some of the accounts that circulate in IR. Einstein makes this opportunism explicit: “[c]ompare a scientist with an epistemologist; a scientist faces a complicated situation. So in order to get some value in this situation he cannot use a simple rule, he has to be an opportunist” (Parascandalo and Höslo, 1995: 117).

When we examine various theoretical positions within social theory and IR, we find this epistemological opportunism at work; theorists use a range of epistemological supports to defend particular knowledge-claims, rather than espousing the vision of epistemological monism that seems to infect explicit debate on epistemological matters. Foucault, for example, clearly placed great reliance on empiricism, whereas Derrida tends toward a rationalist framework. But Foucault bases many of his knowledge-claims on rationalism, just as Derrida employs

empiricist techniques. The same can be demonstrated of any theoretical position within IR. Particular theoretical positions can tend to privilege some epistemological supports over others, but this does not mean that theoretical traditions can be written off as epistemologically monistic. Moreover, even when theoretical positions do privilege one epistemological support, this is the result of a particular ontological or methodological set of commitments, not some a priori allegiance to that epistemology.

Science: theories as maps

Science is a human activity and any model of science must take into account the process by which humans validate beliefs about the world. The positivist vision of science has typically been conceived of as an attempt to mirror the world. This metaphor has been deeply criticized, most notably by Rorty (1979). Observation is not incorrigible; science is not value free; and scientific knowledge is fallible. The mirror metaphor, then, is not adequate for understanding science. An alternative vision, much favored by scientific realists, is that of mapping, or modeling (Aronson *et al.*, 1994; Harré, 1970). I will consider maps to be a particular kind of model and concentrate my attention on the idea of scientific theories as maps of a particular aspect of the world. This point of departure parallels the diagrammatic exposition from Hayes and James in Chapter 6, which calls for greater emphasis on visual representation of theorizing in response to rising complexity.

Science is also a social enterprise. It can be considered as a number of different communities linked by various methods of communication and a commitment to particular norms. But these communities are still constituted (in part) out of individuals. And individual scientists have their own stores of personal knowledge. Nonetheless, the norms of science provide a check on the distortion of science by individual bias. Likewise, all communities are embedded within traditions. Indeed, many religions, for example, explicitly venerate traditional ways and beliefs as belonging to a truth. But for the scientific community, truth does not reside in the past. Rather, it is something to be continually sought, never gained. Truth is an ideal that science aspires to, hence its trajectory is future oriented; truth is a terrain of discovery. This explains why science always treats appeals to tradition with suspicion. In science, tradition is to be questioned. Even when science thinks it is in possession of a truth the realist skeptic sits on its shoulder asking “is this really the case?”

Obviously, scientific communities share with other communities patterns of inequality. Scientific administrators and research leaders have greater power to promote their favored research and often exercise it. Science also can be distorted by outside influence as funders drive agendas and seek those results that maximize their profits. Governments can interfere in science. Access to resources is also internally distorted, and favored journals and editors exercise a control that is not always as open as the norms of science dictate. Jackson draws attention emphatically in Chapter 1 to these biases as an explanation for the virtual

hegemony of neopositivism in IR. Equally, tribal loyalties and allegiances can subvert science.

Science is not perfect, but whereas other communities often support these structural inequalities in terms of tradition, or notions of worth that go beyond the search for truth, they always are treated with suspicion in science. As such, science more frequently rewards young rebels and passes over the favorite disciples. Finally, the notion that contribution to knowledge should be the only criterion of status in science does help to insulate it from the values and politics of the external social system. This insulation is never complete, but the norms of science ensure that it remains a goal. It is the ability of science to remain autonomous from the larger social domain that marks it out (Bourdieu, 2004). As Jackson points out in Chapter 1, scientific claims inherently are impersonal.

Ziman also differentiates science from other domains in virtue of the fact that its products are consensible, i.e., they are capable either of receiving consent or of attracting well-founded objections (Ziman, 1978). One goal of science, according to Ziman, is to achieve maximum consensibility. This property differs importantly from conventionalism, however, because Ziman places reality itself as intrinsic to the way in which consensibility is established.

Consensible theories are models of some aspect of reality. By definition, models are not mirror images of reality. These theoretical representations can be considered as maps that pick out aspects of reality. The analogy between maps and theories is strengthened when one considers how theories are presented visually. Many scientific theories are best represented in diagrammatic form and even mathematics can be understood as a series of symbols.

An important feature of maps is that there is no single correct map of a particular territory. All maps are partial representations that abstract from reality in the attempt to make particular entities and relationships more visible. Abstracting from complexity allows us to identify particular things more clearly.

A map may be drawn to fit the data, and the data collected will vary according to the map produced. Likewise, scientific theories are formulated on the basis of a limited number of observations and the data has a direct relationship to the theory from which it emerges. But a map is not just an accumulation of data; it is an abstract representation organized in particular ways. Maps select features and represent them in a schematic form. This does not make them unrealistic, and it is only if the entities and the relations depicted on the map are captured accurately that the map works. Likewise, scientific theories abstract from the world and produce explanations in terms of the relationships between entities.

Maps and theories also share the fact that their content is directly related to the interest of their producers. Maps and theories, while attempting to represent reality, rely on human input. Both are constructions. Maps are also constructed on the set of background assumptions that are not always clear in the map. Choices relating to orientation, scale, method of projection, and even the symbols to be used, have to be made prior to the drawing of the map, and must be understandable by others who may use the map. Mapmaking requires a body of prior theory and interest related decisions over the choice of appropriate

construction tools. The same is true of scientific theory. The scientist starts with a reasonably clear objective, for example, what entities and processes are to be the objects of study, and what it is they wish to find out about them, as well as a body of background theories and assumptions.

Explaining success: maps, models, and validity

Scientific theories and maps share some important criteria of validity. Although when used in the philosophy of science, valid is often taken to mean “truth preserving,” I use it in its more general sense of “well founded.” This is consistent with my approach to epistemology, which rejects the dichotomy between “truth preserving” and the failure to do so. Our confidence as to how valid (well founded) a theory, or particular claim, is, will depend where on the epistemological continuum we think it sits. According to scientific realists one of the major functions of science is to model reality. Yet, as the discussion of epistemology in the previous section demonstrated, scientific knowledge cannot be known to be true in any absolute sense of the word, but it can be more or less reliable.

My focus here is on whether a scientific theory provides a reliable guide to action; if it does then we can say it is valid. Importantly, in making this move I am not adopting an instrumentalist account of theory validation, but rather, argue that theories provide reliable guides when they offer purchase on the domain that they represent. Realism provides the conditions of possibility for why a theory is a reliable guide to action. Instrumentalists and pragmatists typically attempt to claim utility in terms of practice for themselves. What I show is how a theory, or map, is useful precisely because it has captured something of the realm it is attempting to map or model.

It is important to be clear on the ontology of what is being claimed and the status of the theoretical assumptions under question. I distinguish between “abstractive assumptions” and “entity assumptions” (Wight, 2007). Waltz makes a great deal of the fact that we have to abstract from reality in order to isolate sections of it (Waltz, 1979: 5–8). This is a legitimate strategy and every theory has to engage in this process. When we do so, we make assumptions about the entities in our theories that we know do not fully describe its causal powers and ways of acting. There is no necessary requirement for these assumptions to be unrealistic, unless we take realistic to mean “complete.” Yet, although abstractive assumptions are not complete descriptions of a given realm they are subject to a plausibility clause. An assessment of the plausibility of abstractive assumptions is only possible because they omit properties of a given realm or entity that is already known under certain descriptions. That is, abstractive assumptions do not add new entities to our ontology, but subtract from it in order to allow us to focus on certain elements. Maps are constructed this way, as are social theories.

One example of the use of abstractive assumptions is rational choice theory. Here, theorists assume that individuals can be treated as rational actors, perhaps pursuing self-interest, and disregard all other aspects that might impact on the behavior of individuals (Booth *et al.*, 1993; Green and Shapiro, 1994; Hindmoor,

2006). The validity of this assumption depends on context. In some instances it might be realistic to assume that individuals act in this way, in others not. Note, however, that the issue is one of whether the assumption that the individual can be modeled in this way is realistic in relation to the aims of the theory, not whether the theory provides a realistic account of the individual. A completely realistic account of the individual could not be formulated and, if it could, it would not be a theory but a description.

Consider, for purposes of illustration, Waltz's (1979) exegesis of structural realism. Waltz does not just deploy abstractive assumptions, but assumes something new in his theory. He widens his ontology to introduce the idea that the international political system has a structure that plays a causal role. Let us call this an "entity assumption." The positing of an assumed new non-observable entity to explain a range of phenomena works in a different way from abstractive assumptions. In this instance, we have no prior knowledge of the entity under consideration. Rather than taking away aspects of a given realm the purpose now becomes one of investigating the posited object to check if it possesses those powers suggested by the theory. The aim is not to simplify our account of the posited object but to fill in the details. The discovery of the structure of DNA provides a good example here (Watson, 2010).

This highlights something important about realism in relation to maps and models. It is sometimes thought that realism, with regard to theoretical assumptions, can only mean "an accurate reproduction of the domain under study." Hence, the London Tube map is said to be an unrealistic account of the London Tube system. At one level this is true. Critics raising these issues seem to assume that being realistic entails the production of a copy that is correct in every detail, and a model that fails to do this is said to be unrealistic. This is not what maps, models, and theories are for, and any attempt to address validity in terms of complete replication of a domain is bound to fail. All models, maps, and theories are, by necessity, abstractions from reality, but this should not be taken to mean that these abstractions bear no relationship to the domain they describe. Indeed, the London Tube map is extremely accurate in terms of what it attempts to map. Otherwise it would not be useful.

The London Tube map is a model only of those aspects of reality it aims to represent; the various stations, the differing Tube lines and the relations and connections between them. As most Londoners know, it is possible to leave the Piccadilly Line at Gloucester Road and get a connection to the Circle and District Lines. The map shows no such connection, but it is possible. Those designing the map left out the connection because the lifts linking the lines could only handle limited traffic. This demonstrates that the validity of maps and models is dependent on *both* their accuracy *and* the interests that underpin their design.

Maps, like theories, are abstractions and are constructed with interests and/or problems in mind. Our judgment of how good a map is relates to how well it enables us to carry out the task. Detailed topographical maps, for example, will not be particularly useful to a company searching for diamonds. Maps are selective representations of the world. The contents of the map are chosen

according to their relevance to the problems that they are intended to solve. And because the usefulness of the map can only be assessed by how well it helps to solve the problems of the user, not only is the content of the map interest-related, its validity is interest-related as well.

So far we have two sets of relations that are important in determining validity. First is the relationship between the map and the terrain it attempts to map; second is the relationship between the interests behind the drawing of the map and the ability of the map to meet those interests. Although distinct, I have indicated how they are related, and the ability of the map to meet the interests of those who construct it is related also to the accuracy of the mapping. There is a third dimension.

Although maps are interest-related they are also affected by the background assumptions about the nature of the area being mapped. A geological map constructed to locate diamonds relies on the background assumption that they can be found in deposits of kimberlite and alluvial. If this background assumption is wrong (unrealistic) then any search for diamonds using the map will fail. Each map, then, can be seen to be a model. Models have a relationship not only to their subject (the territory), but also to their source, the interests of those constructing the map. Each map will be a representation of data, selected and organized by the principles underpinning the need for the map. The validity of a map cannot be determined solely by reference to relationship with the territory it maps (accuracy), the interests that led to the construction of the map (its utility) but must also be located in the validity of the background assumptions. That these interest-related features and background assumptions enter into the determination of validity means that the objectivity of mapping cannot lie in the use of any simple truth-preserving mechanism relating a map with its domain.

Nonetheless, valid maps are objective and provide guidance on how to navigate the selected domain. Moreover, maps, like scientific theories, are socially constructed; this means that the validity of a map is a matter for public assessment. Any competent map reader should be able, in theory, to check the validity of a map, bearing in mind the purposes for which it was made. A diamond miner, for example, should be able to predict from an appropriate geological map that a particular terrain will contain the minerals in which diamonds are likely to be found. Their consequent action, to look for these minerals, constitutes a check on the validity of the map, and the discovery of kimberlite or alluvial would validate, at least, that part of the map, and the background assumptions. This example emphasizes the three domains involved in mapping: the data or terrain to be mapped; the map itself; and the assumptions underpinning the mapping. All three are necessary in terms of assessing validity and the map itself is also a function of all three.

Another important aspect of maps, and theories, is that their validity rests on a social consensus that they perform the function they are intended to serve. Ziman argues that it is the nature of maps as abstractions that makes the consensus surrounding their validity possible (Ziman, 1978). The paradox is that the agreement of experts that the map is a good representation is easier to obtain

than would be the case with a photograph. Photographs are representations of what is seen by a particular person in a particular place and time. They are far more detailed than maps. In this sense it is because maps are much less detailed than photographs, but nonetheless objective, that agreement about their validity is easier. But contrary to Chernoff, consensus, or conventionalism, on the validity of the map cannot be the sole determinant of validity, or the test of progress (Chernoff, 2009a). Consensus can be wrong. This argument contrasts with the partially sociological rationale from Chernoff in Chapter 5 for regarding the democratic peace as a success.

The scientific consensus when Galileo developed his theory concerning planetary motion was that he was wrong and John Snow's new theory concerning cholera took decades before it was universally accepted. Indeed, in both cases the scientific consensus attempted to stifle the new theories at birth. Kuhn inadvertently demonstrated just why consensus is such a problematic criterion when taken as the sole measure of validity (Kuhn, 1970). Or, as Ludwig Wittgenstein might have put it:

So you are saying that human agreement decides what is true and what is false? – It is what human beings *say* that is true and false; and they agree in the *language* they use. That is not agreement in opinions but in form of life.

(Wittgenstein *et al.*, 1972, para. 241)

The validity of maps also may be tested. From the London Tube map we can accurately predict which station follows which, and which routes a traveler might take if they were to go from station A to station B. But the issue of prediction does indicate a potential problem with the analogy between maps and social theories. Maps of physical terrain deal with an ontological domain that is *relatively* stable. Social processes, on the other hand, are always “products-in-process” and, as such, display change as constitutive of their being. Maps also, in some way, represent spatial relations as if they were closed systems. Social systems on the other hand are open. Open systems are always causally overdetermined. Even if we have a good theory of one part of social reality, prediction becomes extremely difficult due to the possible interference of alternative interacting mechanisms not considered in the model. However, much depends here on how tightly prediction is defined.

Much is predictable in social life. In societies that use red traffic lights to indicate stop, we can predict with a reasonable degree of accuracy that most cars will stop. What we cannot predict, however, is that *all* cars will stop. Prediction, if it has a role, cannot be given the same status accorded to it in the natural sciences. This also highlights the fact the statements concerning causal laws have to be interpreted as tendencies. However, under the influence of quantum mechanics, and the fact that causal mechanisms outside of experimental closure also operate in open systems, the natural sciences already have embraced such an approach.

Maps (and theories) are connected with other maps, both internally and externally. A map cannot be altered at one point without affecting some of the other

relations in the map and potentially some relations existing in alternative maps. Moreover, coordinates of a feature may be determined with reference to a variety of others, both internal to the map and external to it. So a good map must be internally consistent and any particular feature of the map may be checked with reference to its place in the map as a whole in relation to other aspects of the map. This points to the holistic and relational aspect of validity in maps and theories. One cannot determine the validity of the claim that “Tottenham Court Road” is correctly placed on the London Tube map without determining its relationship to other features on the map; that it is positioned on the right line, that the adjacent stations are correct (and this in turn relies on an assessment of the reliability of their location and relational placement) and that the relationships specified between the lines are also correct. This is the issue of internal validity.

Yet different maps of the same territory also should be compatible, that is, display external validity. This is obviously the case with maps that differ only in scale, but it is also the case among maps that concentrate on different features of the territory. Hence for example, road maps and railway maps. We can combine maps of different features of the same territory, for example a railway map, or a road map, and construct a more general map that includes both features; hence a mapping relationship exists between any two maps of the same territory, and this relationship is embedded within the terrain the map attempts to represent. This issue also highlights why being clear about ontology is so important. In order for a railway map and a road map of a given terrain to be compatible we must assume some overlap of domain. There would be little point in trying to combine the London Tube map with a map of the railway system in Paris. Hence, unless we can establish some common ontological features of different theories there is no point in trying to relate them to one another. Dealing with this issue, however, is rarely as complicated as is often thought. A Marxist explanation of the GFC of 2008 can be related, and possibly even combined, with a neoliberal explanation as long as both can specify the GFC in broadly similar terms. Obviously some translatability of core terms across the theories might be required, but even Kuhn did not think incommensurability fully ruled out translation across perspectives.

Conclusion

The art of mapping is a better metaphor for social theorizing than that of mirroring. Through an analysis of how we assess the validity of maps I have demonstrated that validity is a complex and multifaceted issue. No simple solutions are available and validity takes work. There are no short cuts or panaceas. Validity can only emerge through a complex relational consideration of: (1) the interests that led to the production of the theory; (2) the ability of the theory to provide an accurate account of its chosen domain (tested, at a minimum, in terms of accuracy, coherence, and utility); (3) an assessment of the background assumptions that made the theory possible; (4) an assessment of the external relations of the theory to alternative theories of the same, or overlapping, object domain; and (5)

the assent of the scientific community (although this should be treated with a large dose of skepticism or salt; your choice).

What these all add up to is the point of the chapter. There are no silver bullets, and no grand epistemological standards that make the assessment of validity easy; validity is constructed in practice, but it is a practice that depends on the commitment to realism, and that unfortunately is something neither the positivists nor the post-positivists in IR seem prepared to embrace. In the final analysis, however, the realists need not worry, for despite the disavowal of realism at the level of discourse, it reappears in practice.

Notes

- 1 This is a paraphrase of Drezner's argument, not a direct quote.
- 2 However, I do not, as Patrick Jackson suggests, think that the "ontological turn" can function as a panacea for all disciplinary ills (Jackson, 2011: 27). I do think that reflection on these issues can help orientate research in more productive ways. Probably in vain, I hope that such discussions might facilitate a more integrated form of diversity in the discipline – perhaps even sociable pluralism as articulated in this volume. But this is a long way from claiming that any approach can solve all disciplinary problems. The ontological turn is not a panacea.

3 Substance, form, and context

Scholarly communities, institutions, and the nature of IR

Torbjørn L. Knutsen

What distinguishes scholars of International Relations (IR) from anyone else who discusses international politics? The difference is threefold. First, IR scholars rely on a scientific methodology. Second, they are self-conscious about their place in a theoretical tradition. Third, they are members of a scholarly community.

This chapter will discuss these features of scholarly IR from historical and sociological perspectives to throw light on the nature of IR as a discipline.¹ The chapter first argues that changes in the world trigger discussions in which scholars participate. It then presents an overview of the disciplinary history of IR, arguing that while scholars have discussed international relations for centuries, the field of IR only emerged with the establishment of a community of academics preceding World War I. This new field depended on scholarly institutions that evolved after World Wars I and II. The collapse of the USSR changed the world situation in major ways. It triggered new academic discussions and delivered major shocks to IR scholarship.

Mechanisms of disciplinary change are elaborated at the outset of this chapter. New institutions brought together a community of scholars around World War I – institutions which facilitated scholarly interaction, standardized knowledge, and coordinated academic activities. The chapter then sketches the main lines of the development of IR through the twentieth century. It shows that the development of IR has often been affected by world events and illustrates the role that institutions have played in sustaining the evolving community of scholars. It shows in particular how an increasing interest in international relations evolved during the final quarter of the nineteenth century, spurred by revolutions in industrialism, politics, and education. At the beginning of the twentieth century, a scientific study of IR emerged as scholarly communities evolved along the North Atlantic Rim.

The chapter argues that the “science of IR” was a product of the West – developed by Western scholarly communities and sustained by Western institutions; that the science of IR developed where scholarly communities and liberal institutions of education and research coexisted. It concludes that Western efforts to export IR may encounter challenges akin to those that have met efforts to export liberal democracy. Proper IR, like real democracy, requires a proper

public sphere. Whereas an interest in international relations may evolve anywhere, the science of IR will emerge only in societies where a public sphere is present. The science of international politics will thrive only in open societies whose rulers acknowledge citizens' rights to freely discuss basic political issues and to establish independent unions and scholarly associations.

The basic elements of IR

Where did IR come from? One answer is that IR emerged from World War I. This is the answer of E.H. Carr (2001 [1939]), who saw IR as a reaction to the Great War and who held that idealistic academics founded a discipline designed to establish lasting peace. A second answer is that scholarly discussions about international relations go as far back as antiquity. This is the answer suggested by Hans Morgenthau (1978).

Recent years have thrown doubts upon the first answer and favored the second (Wilson, 1998; Ashworth, 2002; Schmidt, 2002; Hobson, 2012). This chapter, however, will argue that both answers are accurate. The first answer is correct because around the time of the Great War, there emerged university departments and research institutes devoted to the science of international politics. The second answer is correct because scholarly discussions of interstate relations can be traced back to antiquity – to authors like Thucydides.

Events and debates

Scholars have discussed “IR” for hundreds of years. But it took a long time for these discussions to produce an academic tradition. That happened when a self-conscious community of scholars emerged whose members drew connections and established contrasts among various arguments. Carr argued that the Great War drove scholars to discuss causes of war and the preconditions for lasting peace in a systematic manner.

Carr's argument can hardly be the full story. For all wars are, on inspection, attended by such discussions. The Thirty Years War (1618–1648) motivated Grotius, Crucé, and others to discuss the causes of war and to propose ways to establish a lasting peace. Over a century earlier, the Italian Wars (1494–c.1529) sparked similar discussions; Machiavelli and Guicciardini recorded the arguments. Over 1,000 years before that time, the Peloponnesian War (c.431–404 BC) triggered similar discussions – as is indicated by Thucydides. Yet, only World War I produced a would-be *science* of IR. The main reason is that World War I was attended by systematization of scholarly knowledge and by the development of institutions that sustained a professional community of scholars.

Patterns of the past

Scholars who have written about issues of war and peace have always drawn on insights of the past. They have employed the terms and theories developed by

previous authors. Grotius referred to the ancient philosopher Carneades and quarreled with his skeptical views. Machiavelli was affected by Polybius. Hobbes was impressed by Thucydides and translated the *Peloponnesian War*. When G. Lowes Dickinson wondered, in the fall of 1914, why the Great War had broken out, he found his explanation in Thucydides.

All scholars stand on the shoulders of others before them. Old assessments of interstate relations, and past authors' efforts to identify patterns and regularities in the macro-politics of their times, are essential contributions to the knowledge base of the IR community.

Supporting institutions

Scholars who have contributed memorably to past discussions on international relations have relied on patrons. Machiavelli's primary patron was the Florentine Republic. For a dozen years or so he was employed by that city's council; he wrote reports and analyses for its leader, Piero Soderini. After the Medici coup (1513), Machiavelli moved out of Florence and returned to the family farm. Members of the *Orti Oricellari*, a circle of wealthy intellectuals associated with the Florentine Academy, encouraged his political writings. A stipend from the Church sponsored the *History of Florence*.

Hobbes, another historically great figure in IR, was initially hired to teach the young William Cavendish. When William became Baron of Hardwick and Earl of Devonshire, he included Hobbes in his entourage. When William died, the family continued to sponsor Hobbes's efforts to understand social order through a social contract. The influential Grotius worked as a lawyer. He wrote legal briefs for the Dutch East India Company and was a consultant for the Dutch Republic – which used his defense of the freedom of the seas to counter England. He was also at one point Sweden's ambassador to France.

The larger point, brought out effectively via Machiavelli, Hobbes, and Grotius, is that many of the early contributors to IR worked as teachers, consultants) and diplomats. They regularly relied on the goodwill of others.

During the eighteenth century secularization opened up new career opportunities. Advances in printing and in literacy made it possible for writers in philosophy, history, and law to earn a living by the pen. The North Atlantic world saw the rise of text-based media – news-sheets, journals, and pamphlets. In the wake of such developments, there emerged a class of publicists. These were independent writers who owed allegiance to no one and wrote freely.

Many of these writers developed new, original, and often quite critical, perspectives on topics like war, military dispositions, foreign affairs, long-distance trade, and international relations. This is evident in the writings of Voltaire and Rousseau – Voltaire made the claim that monarchies are warlike, whereas a popular-based government predisposes a country for peace; Rousseau insisted that war is a social phenomenon and that causes cannot be reduced to actors' intentions. Hume, inspired by Rousseau, rejected social-contract theories and replaced them with historically founded arguments about the evolution of state relations.

Their arguments were pursued in universities – in subjects like moral philosophy, where students were introduced to subjects like governance, law, and trade. Their insights also affected historians. Some of them wrote about interstate relations in distinct periods. Scottish historian William Robertson (1896 [1767]) included a contextual description of European interstate relations in his book on Charles V. The age of Charles V, he argued, was a formative period in the evolution of the state system and the balance-of-power principle. Decades later, German historian Leopold von Ranke simplified the model of the European states system by reducing it to a few powerful countries. It was the interrelations of “Great powers” which determined the principles upon which the system worked, argued von Ranke (1933 [1825]).

Moral philosophers and jurists tended to discuss the question of international order in different terms. In Europe St. Pierre, Kant, and Bentham made proposals for perpetual peace. In America, Madison (1906), skeptical of the European proposals, adapted social-contract arguments to interstate conditions and developed a theory of federation (Madison *et al.*, 1987 [1788]; Deudney, 2007).

Establishing IR

Speculations about international relations go far back. However, under review they appear too scattered to constitute a scholarly field. Some thinkers wrote texts on war, wealth, peace, and power that became part of the university curriculum of moral philosophy, but they constituted a minor part of a small field. And they did not much affect the decision-making authorities.²

Revolutions of modernity

Although debates had long existed on international affairs and new arguments and proposals steadily evolved, no science of international politics emerged – not until World War I. Why not? There are several answers to this question. Only one of them will be discussed here: the lack of supporting institutions.

Questions of war and peace were debated for hundreds of years, but this was done mostly by thinkers close to foreign-policy decision makers. With the advent of independent writers during the Enlightenment, this changed. With the rise of industrialism and military, economic, and political mass mobilization it changed even more.

First, consider the industrial revolution, which began during the late eighteenth century. It greatly enhanced the capabilities of production and trade of the North Atlantic nations. It made them more wealthy and powerful than others (Buzan and Lawson, 2015). The industrial revolution fueled a Western expansion of shipping, commerce, and credit, and commensurate intellectual influence.

Second, there was the related rise of modern mass politics – of workers’ unions, political parties, interest groups, and civic associations. They provided new channels for political mass-participation. These were concentrated in national assemblies in countries along the North Atlantic Rim, which saw the

consolidation of parliamentary democracy. In that part of Europe, the kings' ministers – including ministers of defense – now had to account for their expenses in front of elected representatives and defend their case in front of budgetary committees. Debates were even reported in newspapers. Thus, the North Atlantic countries were marked by openness and individual liberties; by freedom of speech and by liberty of association. By virtue of that liberty, there emerged a plurality of civic associations channeling the demands and the activism of the citizens (Ostrogorski, 1903).

Wars, discussions, and scholarly communities

A spate of wars erupted in the West during the third quarter of the nineteenth century. The sequence began with the Crimean War (1853–1856), was followed by civil wars in the U.S., Italy, and the Germanies, and ended with the Franco-Prussian War (1871). These were modern wars – fought with industrially produced weapons systems with an intensity and destructiveness hitherto unknown. All these wars triggered huge debates among soldiers, statesmen, and scholars.

The changing character of war demanded new forms of organization, new tactics, and new strategies. In response to rapid changes in weapons systems and politics, Prussia developed a modern general staff, a powerful navy, and global ambitions (Gat, 2001: 341ff.). In the U.S., Alfred T. Mahan emphasized the importance of sea power and developed new theories of its use.

Statesmen were prodded by wars to discuss the consequences of modern warfare. Discussing the ways of meeting a growing need for military manpower, they developed universal male conscription.

Scholars argued the need for new, specialized schools that addressed the changing character of war and preconditions for international order. Most famous among them was the *École libre des sciences politiques*. It was founded in Paris in 1872 as a direct effect of the Franco-Prussian War to introduce French civil servants to diplomacy, law, and political economy. In New York the Faculty for International Politics was established at Columbia University, largely on the model from the *Science Po*. The University of Wisconsin established the interdisciplinary School of Economics, Political Science, and History.³ At Oxford and Cambridge “contemporary history” was emerging as a new field of study. The University of Berlin introduced a course on “Politics” that focused on workings of the state. It was taught by Heinrich von Treitschke and was immensely popular. Treitschke always concluded his lecture series with a discussion of relations among states.

Lawyers, too, began to discuss interstate relations more systematically. One Western university after the other established chairs in International Law. The lawyers' approach was theoretical but with an eye to practical application. When the Institute of International Law (*Institut de droit international*, IDI) was founded in the Belgian city of Ghent in 1873, it discussed ways to establish a robust and peaceful international order through positive law. In 1877, James Lorimer distilled from these discussions a few pithy claims. First, as long as

individual states cherish their sovereignty above all else, it will be impossible to establish any supranational authority; consequently, the interstate system will remain “anarchical.” Second, “anarchy” must not be confused with disorder, because every system of sovereign states has an ordering mechanism in the balance-of-power. If the system is composed of industrial states that are engaged in mutual trade and finance, a second ordering mechanism will evolve: “interdependence.” The most important task in International Law, Lorimer concluded, is to establish a third mechanism of ordering institutions: a system of law sustained by a set of international institutions that can make and enforce the law.

Modern peace associations formulated similar arguments, which emerged after the Franco-Prussian War. These associations appeared in Western democracies as reactions to industrialized wars in the Crimea, the U.S., Italy, the Germanies, and France. These wars had been fought with industrially produced weapons whose destructiveness shocked the masses – the shock being enhanced by the advent of modern newspapers.

The *Workman's Peace Association* and the *International Arbitration and Peace Association* were both established in Britain in 1870. The *Société française des amis de la paix* was founded in France in 1872 – as soon as censorship was lifted after the Franco-Prussian War. Similar associations were founded in all Western democracies during the final quarter of the century and became internationally coordinated.

Establishment of *The Inter-Parliamentary Union* in 1889 was important, because it was an association composed of members of democratically elected national assemblies. The peace movement provided a mass-based medium for systematic discussions of the causes of war and the preconditions for peace. It pointed out the irrational character of modern industrialized war and worked for ways in which conflicts could be settled before the parties began to use destructive violence. The peace movement championed international arbitration (Beales, 1931).

These arguments caught the attention of industrialists. They saw that war destroyed production, disrupted trade, and rendered financial transactions unsafe. Also, as industrial society evolved and trade and finance grew, the industrial countries of the world would be tied together, war would destroy dense networks of interdependence and bring the entire financial system crashing down. Colonial expansion and imperial conflicts triggered political controversies. Colonial wars – like the Boer War – increased war resistance in Europe.

Industrialists and powerful elites, whose commercial fortunes were threatened by conflict, grew preoccupied with questions of war. At the turn of the century they sponsored the growing peace movement. One of the early examples is Swedish industrialist and dynamite manufacturer Alfred Nobel. Other industrialists followed suit. Sir Richard Garton established a foundation to support Norman Angell's writings on war's effect on interdependence – the first fruit of which was Angell's bestselling book of 1910: *The Great Illusion* (Knutsen, 2013). Andrew Carnegie, Edwin Ginn, and others made endowments to sponsor research into the causes of war and peace.

The Great War: discussions and institutions

Before World War I, academic studies of war and peace grew fast. It is interesting to note that this growth was not driven from within the established universities. Rather, major impulses came from outside the university system. The peace movement was an important mobilizer of knowledge about war, peace, and international relations during the final quarter of the nineteenth century. Industrialists and wealthy philanthropists were important sponsors of such knowledge during the first quarter of the twentieth. They funded university chairs, financed peace activists, sponsored study groups, and underwrote research institutes. Initiatives taken during World War I – some by grass-roots organizations, others by powerful, established actors – were particularly important and deserve a closer look. This way of proceeding foreshadows the system-oriented approach of Hayes and James, which in Chapter 6 encourages inclusiveness in the study of causal mechanisms vis-à-vis levels of aggregation.

When the Great War broke out in 1914, it triggered huge discussions and debates. The debates in Great Britain are of particular interest. They paved the way for a scholarly study of IR. One important participant in these discussions was G. Lowes Dickinson, a pacifist and professor of classics at Cambridge University. The outbreak of war spurred him to organize a group of fellow scholars to abolish war – by investigating its causes and then defining preconditions for peace. A similar reaction was that of the Fabian Society, which invited publisher Leonard Woolf to write articles on the causes of war and the preconditions of peace for *The New Statesman*. A third reaction was that of members of the Round Table group, who argued that the key to a peaceful world was education, and who edited a textbook on IR.

What made the reaction in the Atlantic democracies unique was not only that governments tolerated wartime activism that included pacifists and war resisters; politicians and statesmen also reached out to academic activists for information and advice. They seriously considered the peace-related arguments from scholars like Leonard Woolf, John Hobson, and G. Lowes Dickinson.

Hobson (1915) claimed that a robust peace needs a powerful organization of states. Dickinson (1916) agreed, and sought to explain why. Although the war was triggered by the assassination of Austria's Archduke Ferdinand in Sarajevo in June 1914, the ensuing vast conflict had deeper causes, Dickinson argued. One of these was the rapid growth of German power. However, an even more significant cause of the war lay in the anarchical nature of the international system. To establish a robust peace, Dickinson concluded, it would be necessary to harness the international system's anarchical nature. An international organization or a league of states might achieve that goal.

Dickinson's first idea, that war is caused by disequilibrium, had long origins. It had been offered some 1,500 years earlier by Thucydides, and echoed by renaissance-era scholars. Dickinson's second idea, that the source of war was international anarchy, was more original. It caught the attention of the British Foreign Office, who invited Dickinson's group to participate in a committee to discuss the nature of a postwar peace.

The group studied past proposals for perpetual peace by authors like St. Pierre, Rousseau, Kant, and Bentham. Their discussions were observed by Robert Cecil, Undersecretary of Foreign Affairs in the wartime government of Lloyd George. Cecil wrote reports on the committee's work and kept the cabinet informed. He also made sure that some of the group's ideas were sent to U.S. President Woodrow Wilson. In 1917, U.S. President Wilson established a comparable group, "the Inquiry." The French government also established a group to help prepare the peace conference which was bound to be organized after the war. This connection between academics and executive powers in democratic states was an important step in the institutional evolution of a science of international politics. The institutionalization of IR can be traced further back than World War I – e.g., to the reactions to the Franco-Prussian War with its founding of specialized schools, the growth of modern international law, and of the international peace movement. However, this development gathered speed during World War I, as Atlantic states prepared for the postwar peace conference. Then it obtained a critical mass at the peace conference itself.

The conference was huge. Diplomats from the belligerent countries showed up with big delegations. Among their number were many academics – historians, geographers, jurists, and political scientists – who were brought along as advisers. While the diplomats negotiated, the experts would meet in Paris cafés to exchange views on the negotiations. Upon their return, they would often champion the establishment of chairs and schools of international politics.

IR does not have a birth date. But May 30, 1919 may be its moment of conception. On that date about 20 academics, chiefly from the British and the U.S. delegations to Paris, gathered at the Hôtel Majestic. They agreed that regardless of how the peace conference turned out, there would be a need for institutes where academics, diplomats, lawyers, and political leaders could meet to discuss affairs concerning war and peace. This discussion at the Majestic was followed up by initiatives on both sides of the Atlantic (Shotwell, 1937: 346). In 1920, the British Institute of International Affairs opened in London. In 1921, the American Council of Foreign Relations opened in New York. These became models for others. During the 1920s, similar institutes were established in Germany, Poland, Denmark, Canada, and Hungary.

The spirit of Paris was apparent in the scholarly world. The years which followed Versailles saw several chairs and schools emerging in Britain. One of the first was the chair of International Politics at the University of Wales, Aberystwyth in 1919. In the U.S., the Edmund A. Walsh School of Foreign Service was established at Georgetown University – also in 1919. Half a dozen schools devoted to IR quickly followed suit, to say nothing of departments and institutes.⁴ U.S. universities added several hundred new courses with international relations themes during the 1920s.⁵ They all tended to discuss war and peace – with a particular focus on the Paris Conference (Knutsen, 2016).

The most important institution created in Paris was the League of Nations. It became important not only because it was a forum for diplomats, but also because it offered a meeting place for scholars. One of its missions was to stimulate

research and education in international cooperation and peace. For that purpose it set up international organizations like the International Committee on Intellectual Cooperation (ICIC). One of the tasks of such organizations was to identify institutes, universities, and schools that taught international subjects, contact them, connect them, and coordinate and standardize their activities. Another task was to establish more institutions of research and education. A third was to arrange meetings and conferences to discuss international issues and create contacts among scholars.

These activities were run on a shoestring and progress was slow. Speed picked up at the end of the 1920s, after the ICIC established the International Studies Conference (ISC), which received financial support from the Carnegie Endowment for International Peace and from the Rockefeller Foundation. In 1928, the ISC arranged its first annual conference in Berlin and brought together IR scholars from Europe, Britain, and America.

The ISC's fifth annual meeting, in Milan in 1932, was a turning point. The ISC decided to arrange future conferences around a particular theme. Also, it determined to appoint a rapporteur to write the minutes for each conference and account for the papers and discussions. These decisions focused and systematized the activities of the ISC. Central members of the ISC – Paul Mantoux, David Mitrany, Pitmann Potter, and Arnold Wolfers – had been important contributors to this development.⁶

The origins, nature, and evolution of IR

The science of international politics emerged from World War I. Yet, its roots go far back. A host of past scholars – Thucydides, Machiavelli, Hobbes, etc. – have contributed to a vast store of knowledge about war, wealth, peace, and power. Their texts are the raw material out of which a tradition of IR could later be fashioned. A vision of a vast, virtual community of scholars has been expressed in various ways by various scholars – by Karl R. Popper (1978) who formulated the idea of a “world 3” of objective knowledge, Michel Foucault and Jacques Derrida who expanded the concept of “the archive” (Manoff, 2004), and Pierre Boyle, who long ago developed the notion of a “republic of letters.”

Conversations and debates

This vision of a timeless community of scholars gives meaning to the fashionable notions of “interrogating” the past and of “conversing” with ancient authors. However, fashionable does not mean original. Machiavelli claimed to have written his *Prince* on the basis of interrogations of past statesmen and conversations with ancient scholars. He explained in a letter how he prepared for these conversations. When evening came, he would remove his working clothes and wrap himself in “robes meant for a court or palace.” Then, dressed appropriately, he entered his study. Or, as he wrote:

I enter the ancient courts filled with ancient men, [where I am] affectionately received.... I am unashamed to converse and ask them to explain their actions, and where they, kindly, answer me. And for four hours at a time I feel no boredom, I forget all my troubles, I have no fear of poverty.

(Machiavelli, quoted in Unger, 2011: 214)

All scholars converse with their forebears. All stand on the shoulders of others. They draw on earlier discussions and arguments. They agree with some of them and claim to refute others. Once a scholar gains some acquaintance with these conversations, she will be hard pressed to find an argument or an angle that can be considered novel or original.

During the late nineteenth century, scholars began to approach older scholarship in a more systematic fashion. They traced patterns in previous arguments and made connections among past authors. At this point the scholarly IR discussions altered. In addition to debates about individual observations and arguments, there evolved a more abstract debate about approaches or schools. Treitschke, for example, emphasized the importance of Machiavelli and extolled him as the originator of the state-centered approach. Other nineteenth-century authors favored the contributions of Grotius or Locke, celebrating the development of contract theory as a conceptual breakthrough. This sparked a debate about the merits of competing approaches.

A telling example erupted in 1912 between Norman Angell and Alfred T. Mahan. Angell had just published his *Great Illusion*, a book which viewed world affairs from the point of view of interdependence theory and which warned that *Realpolitik* might well lead the Great Powers of Europe into an all-destructive war. The book skewered the Realist approach as a net contributor to war itself (Knutsen, 2013). Mahan, who considered himself a Realist, saw a need to defend his tradition and wrote a scathing review of Angell's book. The bitter exchange which ensued between Angell and Mahan on the eve of the Great War was a discussion between a Realist and a proponent of interdependence theory.

This early exchange, a "First Great Debate," shows how each of the two scholars is self-consciously aware of belonging to a particular tradition of international scholarship. It shows that the two traditions were already established and scholarly "canons" of IR existed *before* World War I – i.e., before organizing a self-conscious and tight-knit community of IR scholars around institutions of research and education. A community of scholars, unified by traditions and canons, emerged roughly together and in turn stimulated the development of institutions for research, education, and coordination of scholarly activities.

The establishment of scholarly traditions was part of an emerging standardization of knowledge that was later formalized and that provided an early common identity to scholars whose activities lifted up an interest in international affairs to a science of international politics. At this point another issue entered the IR discussions: a discussion of what science meant and what a science of international politics entailed.

IR in a nutshell

E.H. Carr (2001 [1939]) was right when he claimed that IR was born under an idealist star in the 1920s but became increasingly dominated by a Realist outlook as the 1930s progressed. However, he exaggerated the degree of change involved. First, it is worth recalling that the 1920s was not exclusively Idealist – e.g., Mackinder published his *Democratic Ideals and Reality* early in 1919 as a Realist warning to the diplomats who prepared to negotiate a new world order at the Paris Peace Conference. Second, the late 1930s were not entirely dominated by the Realist perspective. This is indicated by the conference themes that the ISC chose during the years leading up to World War II. In 1934 and 1935, they selected “collective security” as their theme. In 1936, 1937, and 1938 they chose “peaceful change” – a choice of theme which does not immediately support the notion of a scholarly community embracing the Realist outlook. The 1939 conference was canceled due to the outbreak of World War II.

The poles of Idealism and Realism

Idealism did not fade from view during the 1920s and 1930s. Neither did Realism gain total dominance. The distinguishing mark of interwar IR is of a discipline torn between Idealism and Realism. If early IR authors presented Idealism and Realism as “poles,” they did not think in terms of absolute opposites – as in “north pole vs. south pole.” Rather, they tended to think in terms of pillars or posts – as in “supporting poles.” Thus, interwar IR may be seen as placed in a hammock, suspended between an Idealist pole at one end and a Realist pole at the other; both of them necessary to uphold the scholarly field. The historical analysis of Realism was tempered by the understanding of norms, habits, and rules implicit in the legal approach (Mackinder, 1919).⁷ The idea that Realism and Idealism are mutually exclusive approaches emerged during the Cold War.

World War II and the evolution of IR

World War II triggered big debates that tended to orbit around old themes. Some were Realist; others Idealist. Realism was not preeminent – neither during the late 1930s nor during World War II. Wartime discussions included ample arguments of an Idealist nature. This was reflected in President Roosevelt’s wartime diplomacy and evident among academics who surrounded the Roosevelt administration.

Roosevelt made it an important part of his wartime diplomacy to build institutions designed to stimulate cooperation after the war. His liberal-internationalist efforts were visibly supported by Vice President Henry A. Wallace and U.S. Secretary of State Sumner Welles. The Realist argument had been expressed by Churchill, Nicholas Spykman, and others during the war. It made its final breakthrough in U.S. policymaking circles with the famous formulation of George F. Kennan in 1947. Kennan argued that (1) negotiation with

Stalin was futile; (2) the Soviets would reject Western openness and market-based interaction; and (3) a Soviet communist would not support a peaceful world based on liberal premises. Hans J. Morgenthau agreed. He made a hard-hitting criticism of Roosevelt and his foreign-policy academics. According to Morgenthau, none of them understood that there is a deep difference between politics within states and politics among states, or had the faintest idea of international politics. They were blind to the realities of power, which in turn produced “a fundamental misconception of what foreign policy is all about” (Morgenthau, 1948: 127).

Cold War IR

Morgenthau (1948) totally rejected the liberal internationalism of the Roosevelt administration. He opened up a new understanding of the relationship between Realism and Idealism. He did not see the two as mutually supportive or dialectically related. He saw them as incompatible. This view defined IR for a generation.

IR scholars continued to address questions concerning the causes of war and peace. Their arguments were colored by postwar developments. One of these was a new atmosphere of cooperation that emerged among Western European states living in the shadow of the USSR. When IR scholars discussed Western relations, they focused on peace and followed the logic of liberal internationalism. They discussed European integration, Atlantic cooperation, security communities, and democracy (Deutsch, 1954; Mitrany, 1965).

Another development, the Soviet bomb, informed Western discussions on war. These tended to focus on East–West relations and flow from Realism. These discussions encouraged a new sub-discipline: strategic studies. Although it sprang out of Realism, strategic studies as a field developed its own theories of balance and deterrence. It evolved a new, hyper-rational form of game theory developed by mathematicians, economists, engineers, and the American RAND Corporation. They challenged IR’s traditional approaches which were rooted in historical examinations, philosophy, and law. They presented rational-actor models and systems analyses.

A methodological rivalry opened up in the social sciences between old and new research methods. As computers were introduced, the ability to convert data into numerical form and to process it routinely, revolutionized the social sciences. It triggered bitter methodological debates between advocates of the traditional methods and behavioralist techniques. These arguments were, essentially, a clash between two philosophies of science – one traditional, contextualizing approach informed by History and International Law, the other a correlational approach based upon the hypothesis-testing logic of the natural sciences. IR was shaken by deeply divisive methodological discussions because there was a great distance between the traditional approach and that of behavioralism. Also, the rift between the two was complicated by a new international phenomenon: decolonization.

IR and decolonization

The West had dominated world affairs for 400 years. Two world wars had weakened the colonial powers and shaken their sense of moral superiority. As Western powers sought to regain control over their colonies, they were defied by indigenous peoples who demanded an end to colonial rule. They referred to Western involvement as imperialism. Western powers, they argued, had conquered overseas territories by force, exploited and impoverished their economies, oppressed their populations, and corrupted their traditional systems of rule.

At first, the colonial powers brushed such charges aside and tried to regain their old influence – often through force. It was soon evident, however, that rebel resistance was more robust than assumed. Also the colonial powers were increasingly out of step with world opinion. Colonialism was condemned by the USSR, whose communist leaders saw imperialism as a product of capitalism and who supported the rebel cause. The UN General Assembly also condemned colonialism and passed declarations which supported decolonization.

This put the U.S. on the horns of a terrible dilemma. Americans traditionally had opposed colonialism and supported decolonization. But they also worked to contain Communism. When the USSR began to support Third World movements of national liberation, the U.S. might, if they followed their anti-colonial instincts, end up assisting the communist cause. Besides, if the Americans acted on their anti-colonial instincts, they would be at loggerheads with their European allies. So, while the USSR supported Third World wars of national liberation, the U.S. ended up supporting the status quo. Determined to contain Soviet expansion, the Americans threw their weight behind European allies who fought insurgency movements. They backed repressive Third World regimes to fight radical rebels. The result was a groundswell of limited wars that washed across the Third World. The USSR and the U.S. both picked sides in these wars, subsidized the belligerents and made them longer and more deadly.

During the 1960s, the world witnessed a steady accumulation of what might be termed Third-World wars. Also, world opinion engaged in debates about wars of liberation and wars of revolution, about political independence and economic development. These debates also engaged IR scholars. Some of them embraced the radical, Third-World version of the argument and criticized Western colonization. By the mid-1960s, new revolutionary arguments hit the IR community with full force. Advocates used macro-historical models and theories of political economy to critique both Realist and Idealist thinking. By 1970, three competing approaches or traditions vied for influence in the IR community: to the old Realist and Idealist approaches was now added a new radical or revolutionary approach – or a new “paradigm,” to use the then new and fashionable term. The new paradigm – later crystallized into the vast and amorphous movement toward critical IR – challenged positivist thinking associated with conventional approaches.

Around 1990, another dramatic turn of events shook world affairs: the exhaustion of the USSR and the end of the Cold War. These events raised new issues and triggered new discussions which affected IR.

Post-Cold War IR

By the time the Cold War ended, IR had been consolidated as a field. It was studied at universities around the world on virtually all continents. Again, IR adapted to the times. It still discussed war and peace – although “new” and different wars were now in focus: wars cast in ethnic and religious terms. IR further broadened its agenda to include other issues.

One set of issues reflected older questions of collective identity: from previous discussions of nationhood and class now evolved concern with ethnicity, faith, language, gender, and sexual practice. Another set of issues concerned the planet. The time-honored question of how resource scarcity relates to conflict and war was expanded. But the greatest expansion concerned themes relating to order and welfare. New questions began to be asked from the point of view of a planetary environment: about how the extraction of resources relates to human industry and welfare; how to acquire and distribute enough energy and food for the earth’s inhabitants to remain productive, fed, and healthy. Indeed, how to ensure the health of the planet itself became an ongoing question. A third set of issues concerned technological innovation. The question had preoccupied political economists, contemporary historians, and geopoliticians at the end of the nineteenth century. A century later the question re-emerged with full force in IR, stirred by new discoveries in metallurgy, developments in electro-magnetism and miniaturization, and new designs in digital communication systems. Satellites, computers, and a host of other innovations created new priorities for IR as a discipline.

Events, debates, and patterns of the past

New technologies opened up unprecedented possibilities to collect, store, retrieve, and manipulate text and images. They triggered a communication revolution which changed relations among people and altered financial and diplomatic relations. The new networks could transmit messages everywhere. They were fast and cheap. PCs, cell phones, and social media networks affected everyday life. They changed banking. They revolutionized entertainment. They opened access to information. In short, they transformed people’s perceptions of the world. And they changed relations among states by stimulating international coordination and cooperation.

The new communications technologies opened up entire societies. This struck hard blows against autocratic states by undermining their monopoly of information. Yet the new technology also opened up new possibilities for surveillance, espionage, sabotage, and warfare (Greenwald, 2014).

On balance, the new technologies favored the open societies of the liberal democracies of the West. Technological innovations boosted a revival of liberal values and democratic ideals which washed around the world. This revival had already begun in the Atlantic states in the 1970s and spread from there. During the 1980s a wave of democratization swept Latin America and the Catholic

world in particular. By the end of that decade it had reached the communist world, where it boosted demands for political reform. China adapted to the changes; it adjusted its economic system to suit the market economy. As a result, it experienced a remarkable streak of economic growth. By contrast, the Soviet Union tried to adapt economically and politically. It failed on both accounts. The Soviet empire unraveled and the USSR collapsed. The radical or revolutionary tradition of IR theory, which had at least some connection to the Soviet Union as an exemplar of socialism, collapsed as well.

Technological innovation, the rise of China, and the collapse of the USSR altered international relations. These big events triggered big discussions. IR scholars sought to understand the forces of change and come to grips with the new international situation. The early 1990s was a period of dynamism and innovation in IR.

These efforts toward theorizing met with mixed success. Some of the boldest efforts were speculative and abstract and were criticized for being unscientific. These efforts would correspond to viewpoints outside of neopositivism, and within the other three approaches, as enumerated by Jackson in Chapter 1. Other scholars cleaved tightly to science; they were more prone to recycle ideas from the past – especially ideas from the formative decades preceding World War I. They observed how trade and travel were stimulated by technological innovation – echoing old observations from the 1890s, whose scholars had commented on how the West expanded its global influence and how this improved the non-Western world with human rights, democracy, development, and international law (Leroy-Beaulieu, 1874). One century later, post-Cold War IR scholars made remarkably similar claims, often echoing James Lorimer's (1877) vision of norms, rules, and laws as ordering institutions in IR.

The most salient discussion of the 1990s may be summarized in the two most fashionable terms of the 1990s: "globalization" and "democratic peace." It is useful to place them in a wider historical context by reviewing what might be identified as the larger theoretical momentum of the discipline.

During the course of the twentieth century, IR established itself as a discipline. The community of IR scholars responded to events and engaged in debates about war and change. Each big debate shook the scholarly community and made its members adjust focus or change direction. IR developed through turns and spurts rather than through a steady accumulation of knowledge.

However, IR did not evolve willy-nilly. It retained some constant core features. For all its changes and diversity, IR tended to cleave to a few basic conceptual schemes. First and most significantly, IR retained the view that a system composed of sovereign states is basically lawless or anarchic. Second, IR continued to distinguish anarchy from chaos and to claim that the anarchic system of states possessed mechanisms which imposed order upon the lawless condition. The primary source of order has long been identified as the balance-of-power – as suggested by scholars from antiquity like Thucydides, repeated by Renaissance scholars, and endlessly elaborated ever since. Over time, however, other mechanisms of order have been added. During the seventeenth and eighteenth centuries,

scholars discussed the effect of common Christian norms, of diplomacy, and international law. During the nineteenth century scholars also noted the effects of interdependence.

At the threshold of the twentieth century, different scholars emphasized divergent principles of order. Treitschke (1963: 294f.) divided scholars in two groups. On the one hand were those who emphasized the ordering effect of power (what he called “the naturalistic” approach of Machiavelli and Ludwig von Rochau). On the other were authors who argued that order emerged from trade among free and rational actors (which Treitschke called “Liberal theorists,” free-traders, and followers of Richard Cobden). The two approaches were aptly illustrated by the debate between Alfred T. Mahan and Norman Angell on the eve of World War I – where Mahan emphasized the importance of power and Angell that of reason, trade, and independence. Note that, from the point of view expressed by Wight in Chapter 2, the debate came down to differences over unobservables of the kind associated with scientific realism.

However, these two approaches by no means exhaust the arguments in the vast republic of letters. Many scholars have drawn on arguments from the tradition of moral philosophy. Every once in a while there has emerged a third, unstable and changing approach. It has appeared and then faded, sometimes drawing on insights from political economy about the division of labor in society (as expressed by authors like Jean-Jacques Rousseau or Adam Smith), sometimes on arguments from legal philosophy about habits, customs, norms, and rules (as conveyed by authors like David Hume). The most recent incarnation of this tradition has been the post-Cold War discussion of how ordering mechanisms have resulted from communicative acts and human interaction itself. These arguments have imported insights from Sociology, Anthropology, Law, and Linguistics.

After the end of the Cold War, moral philosophy reasserted its influence in IR. As the world changed and extant paradigms could not account for the changes, insights from political economy and legal philosophy re-emerged. This is most apparent in the case of the debate on globalization. Scholars tended to observe that a communications revolution had altered world affairs and to develop arguments in several directions. Three arguments stand out. One of the first arguments applied political-economy arguments to increases in commercial interaction. Discussions of global divisions of labor, transport, trade, and finance revived arguments from the old interdependence theory. A second argument related to travel – to the transport of people, including waves of migration. One body of arguments concerned the migration of people from the South and the steady accumulation of immigrants in the great cities of the North. These cities quickly evolved into multiethnic and multicultural communities. A third argument concerned the new movement of information. Suddenly, the world was awash with information. Much of it is interesting, absorbing, and engrossing. And it is easy to forget that information is not really knowledge.

When the Cold War ended, the orthodox radical approach within IR collapsed. This raises two questions. The first is whether the swiftness of its

collapse indicates that the original rise of radicalism was sponsored or at least encouraged by the USSR and its Third World allies. The second concerns the new approaches that emerged to fill its void and whether these approaches are upheld by states and other powerful actors which have risen to make their mark on the post-Cold War world (Choucri, 2012; Greenwald, 2014).

The communications revolution has produced an unprecedented access to data and sources of information. Those sources can be purposefully mined and converted to useful knowledge. This raises several methodological questions in contemporary IR. Some questions concern the prerequisites for obtaining information. Others concern the conversion of information into useful knowledge.

An unprecedented amount of information is now available in digitalized form. To access this rich store of information it is necessary to have, first, a computer. Second, it is necessary to know the geography of the Web and have skills to navigate it. A third requirement is a command of English. IR has traditionally been an English-language discipline. In recent years English has further consolidated its position as the dominant language of the field.

The incoming IR student has more information at her fingertips than most scholars had in a lifetime. To be able to locate information that is relevant, reliable, and valid it is necessary to know what to look for and to have the skills to find and assess it. It is necessary, therefore, to be well versed in theory and methodology. It is clear that theory and methodology advanced significantly during the fertile 1990s. Traditional research methods were enriched by new data processing techniques and advances in social philosophy. The ability of interrogating the past was, for example, sharpened by discourse analysis. Also, quantitative methods improved – partly by development of new statistical techniques, and partly through user-friendly statistical packages.

After the Cold War, the field of IR embraced many new issues. With new issues came impulses from other fields, such as Geography, Sociology, Anthropology, and even Literary Criticism. This brought new social philosophies and new methodological angles to IR. These enriched and diversified IR. But there were also costs.

Here are two costs to consider: first, there is theoretical overload. Post-Cold War IR has acquired so many diversities and subdivisions that it is hard to make sense of them. Incoming students are faced with many traditions and with subdivisions within them; it is quite a change from IR during the Cold War, when beginning students were introduced to two or three main theoretical traditions (Wight, 1991). Additionally, students now face specialized subject areas, each with demands to master more complex research techniques. This suggests a second cost: loss of substance. Beginning students are presented with more formal demands – in the form of computer skills, research methods, types of approaches, and a growing body of theories – and less international politics. More skills and abstractions, but fewer big events, are encountered from the outset in IR.

Diplomatic history, for instance, used to be part of IR's inheritance. It introduced students to political agency and chains of events, including major crises and their resolution. It also introduced students to states' habits and agreed-upon

norms of interaction. Diplomatic history, as Kissinger (1954) once noted, “is the memory of states.” It is noteworthy how IR has downplayed events and diplomatic history in recent years and replaced it with abstract philosophies of communication and of social interaction. It also is odd, because if it is true that IR is significantly shaped by events, it would make sense to pay more attention to the events that shape it and less to the accumulating variety of specialized theories that are shaped by them.

Why does IR now put less weight on events and diplomatic history? One reason is the old culprit, “the arrogance of the present.” It often follows on the heels of big, game-changing events and pulls the following argument in its train: the world has changed profoundly and we can no longer learn much that is valuable from earlier times.

This old argument is powerfully present in post-Cold War IR, where it is often seen dressed up in two sets of new clothes. The first is tailored from the cloth of technological innovation. It is a product of our digital times and hinges on the claim that there is no longer much need to know about events of the past because all necessary information is now readily available on “the Net.” This is a shallow observation which confuses information with knowledge. Although it is correct in pointing out that all kinds of information is available on the Net, this viewpoint forgets that only information which is processed and stored in human minds counts as knowledge and that such cerebrally kept knowledge is the precondition for thinking and theorizing.

The second new variant comes from a postmodern revival of radical arguments. Its vantage point is that knowledge is power and that Western knowledge has exerted influence and discipline on the non-Western world. These are reasonable claims. The science of international politics is of Western origins. IR has its main roots in countries around the North Atlantic Rim. There can be little doubt that IR is a Western product. There is little doubt that IR has drawn on knowledge from other cultures (Hobson, 2004) – and that the republic of letters is not exclusively Western. Yet, IR has been shaped by Western events and has been tailored to answer Western questions and meet Western needs. This reality is both acknowledged and emphatically critiqued at length by Sjöberg in Chapter 9.

Some post-positivist authors have used this insight to criticize established IR for being culturally biased and unsuited for the multicultural, post-Cold War world. Others go a step further and call for a replacement of hegemonic Western IR with local, non-Western alternatives, arguing for the desirability of establishing a post-Western IR.⁸ This is idealism in the tradition of the American and French Revolutions and of German Romanticism. It is based on the ambition “that every human being shall live a full and self-respecting life” (Mackinder, 1919: 7).⁹ Chapters 8 and 9, from Ish-Shalom and Sjöberg respectively, follow from that point of departure and pursue alternative modes of thinking inspired by normative theory.

This new form of idealism is countered by the Realist question of whether the necessary preconditions exist for scholarly communities to be organized in non-Western societies and for political theorizing to take place there. Idealism tends

to assume away practical questions pertaining to resources and power (Mackinder, 1919: 7); it assumes out of hand that a scholarly science of international politics can take root anywhere. The Realist would point out that it cannot. History is rich in examples of rulers who have quashed political discussions – be they scholarly or not – because they may question the rulers' authority and undermine their rule.

There are many countries in the contemporary world where political theorizing is a hazardous discipline. One example is China, whose leaders clamp down on discussions about human rights. Other examples include countries like Burma, North Korea, and Uganda. In these countries individuals shun political debate. Political associations will not spontaneously emerge here. Communities of scholars are unlikely to appear. And if they should emerge, private enterprises are unlikely to step forward to sponsor them. In short, none of the institutional preconditions for a science of international politics exist in these countries. If a post-Western IR were to develop, it is unlikely to happen under conditions of dictatorial rule. Rather, the optimal conditions for the development of a scholarly community of post-Western IR are found in the West and open societies in other locations.

Conclusions

This chapter has discussed the nature of International Relations (IR). It has sketched an outline of history, arguing that speculations about international relations can be traced far back in history. It has noted that such speculations have clustered around discussions about big events, such as the beginning and ending of war.

The chapter has noted that the modern, systematic discussions of the causes of war and the preconditions for peace emerged on the heels of a cluster of mid-nineteenth century wars. It has argued that institutions – such as schools, institutes, and the modern peace movement – were established in the wake of these wars and that they have played important supporting roles in the development of modern IR. These institutions sustained a growing community of scholars and sustained their efforts to systematize earlier international discussions into an academic canon.

The chapter has argued that modern IR emerged in countries along the North Atlantic Rim during the final third of the nineteenth century, the product of a particular postwar confluence of industrialization, economic growth, mass mobilization, and international expansion which transformed the West and the international order (Knutsen, 2016; Buzan and Lawson, 2015). The chapter has insisted that IR, since its inception, has been affected by contemporary international events. That the discipline has been jolted by wars and crises that have altered the focus of scholars who have shifted their emphasis to address the issues of their day. It is no coincidence that the science of international politics emerged after World War I. During that war, IR scholars tended to train their attention on questions of how to build a durable peace. IR was once more shaken

by World War II around 1940 and was then consolidated as an academic field. IR scholars then added the concern of how to maintain a stable balance between two superpowers. Their task was complicated by the process of decolonization and Third World wars. These events increased the number of states in the system and altered the system's dynamics. At century's end, the unraveling of the USSR shook IR scholarship once more. The radical or revolutionary approach to IR was brushed aside. As the Cold War ended, IR scholars again began to discuss ways in which it was possible to build a durable peace.

The world turns and things change. IR scholars have always observed the changes. They have allowed them to affect the questions they ask and to drive their scholarly discussions. Under such shifting circumstances, it is hard to maintain a fixed focus for long. Affected by international events, it is hard for IR to conform to established scientific ideals of progress as accumulation of knowledge that is universal and certain.

If IR scholars find it hard to fix an objective reality and run standard tests against it to accumulate knowledge, how does the discipline evolve? If it does not evolve through a steady accumulation of knowledge, does it instead evolve through "scientific revolutions" and "paradigm shifts" (Kuhn, 1970)? Not really. The standard notion of scientific revolutions assumes that old paradigms are replaced by new ones. But this does not happen in IR. Old IR paradigms never die. They may fade. But they also tend to accumulate over time.

In the wake of the Soviet collapse, some IR approaches faded away – the radical or revolutionary paradigm being a case in point. At the same time, the end of the Cold War inaugurated a particularly fertile period in IR theorizing. New theories accumulated faster than the old ones faded. Post-Cold War IR became a bit like a roach motel in the end: theories checked in but they didn't check out.

The main way to prevent IR from accumulating endlessly – and from suffering theoretical overload – has been for IR scholars to allow old theories to quietly fade away. This mechanism has been sustained by an arrogance of the present and a tendency to focus on current events. IR scholars have refrained from looking back. They have favored contemporary questions and new, progressive techniques. The result is a discipline with an attention deficit disorder. Scholars produce steadily new arguments. But how far do they stray from the old? How often are novel theories variations over older, basic themes? In the wake of the Soviet collapse, for example, constructivism was seen as a novel approach; it could also be seen as an effort to reinstitute the old – to rescue the methodology of classical approaches from the clutches of reductionism, economism, and the positivist ideals of scientism (Ashley, 1984).

After the end of the Cold War, IR scholars produced seemingly endless variations over old, liberal-democratic themes. They emphasized, for example, the universality of human rights, of reason, and of individual agency. The result was a neo-Idealism which tended to assume that peace and democracy are inseparable; that democracy is the source of all good things and that where democracy is established, peace and other good things will soon follow. Scholars from

critical IR, such as Sjöberg in Chapter 9, quite rightly view this triumphalism with suspicion.

Assumptions about the value of democracy were held commonly by the very first IR scholars, too. These views were broadly accepted among the peace advocates around 1900. A few years later, in the wake of World War I, champions of the novel profession of IR repeated these pro-democratic notions but in a new institutional context. They emerged in democratic societies which had recently been threatened by autocracy in war. Democracy was, in fact, an existential prerequisite for the new discipline. Without it, a community of IR scholars would hardly emerge and professional associations would scarcely evolve. While democracy may not be a guarantee of peace, it could be a necessary condition for mutually respectful and constructive dialogue.

The pioneers of IR argued that the study of international relations would help consolidate democracy – that IR would stimulate discussions about world affairs and help educate the voting public; that it would disseminate knowledge of politics to the public and help voters make well-informed and rational choices at the ballot boxes. Also, IR scholars would provide statesmen with knowledge, obtained through the use of scientific methods, about the causes of war and the preconditions for peace. Such public services would not be limited to their own countries; they would benefit the world.

Early IR scholars considered democracy to be a key ingredient in peace. They tended to assume out of hand that democracy could be exported – and that IR could be studied everywhere. Some, however, were less certain. Among the skeptics was Paul S. Reinsch, who argued in 1905 that headless efforts to export democracy were unlikely to succeed.¹⁰ He would agree that ideas about politics – international politics included – may occur anywhere. Thoughts and speculations of the private kind – like the nocturnal conversations that Machiavelli maintained with imaginary courts of ancient men – may be found in many places; and if written down and preserved, they may add to the store of knowledge available in Popper's "world 3" of objective knowledge or in Bayle's republic of letters. However, only *public* conversations can fashion a community of scholars. Such a community requires freedom of speech. And the institutions and associations that support it are created under conditions of freedom of association. The establishment of a science of international politics, in short, requires an open society and a working public sphere.

Countries around the North Atlantic Rim have long been maritime trading states. Early political theorizing was regularly sponsored by private wealth, often by merchants who would profit from conditions of order and peace and who would face uncertainties and loss in times of conflict and war. The commercial elites, who had an interest in maintaining freedom of association and in keeping themselves informed about relations of power, at home and abroad, also developed an interest in issues of war and peace.

It should come as no surprise, then, that IR is a Western discipline. It emerged in societies around the North Atlantic Rim, hand in hand with modernity and with the political and economic mobilization of the popular masses. Early

academic explorations into the causes of war and the preconditions for peace were part of a public discussion and took place in an expanding public sphere, marked by freedom of expression and assembly. They took place within a framework of tolerance which allowed citizens to question the wisdom of their politicians. It allowed them to criticize, even to mock, their leaders, laugh at their kings, and to live to tell about it. Ideologies may evolve anywhere. Political theorizing can evolve publicly and with sustained value only in societies where liberties exist. The same, perhaps, can be said of sociable pluralism – this volume’s unifying concept – in the context of exchanging views about philosophy of science in IR.

Notes

- 1 A discussion of these features and their connection to broader methodological debates in the social sciences is found in, for instance, Moses and Knutsen (2012: 183ff.).
- 2 Most rulers paid little attention to the arguments of the moral philosophers. Even an intellectually alert and enlightened monarch like Prussia’s Frederick the Great relied on prudence, his own sound sense, and royal classics – as expressed, for example, in the literature of “kings’ mirrors.”
- 3 Frederick J. Turner taught at this school for several years. So did his student Paul S. Reinsch, who also taught a class on World Politics in the school year 1899–1900 – and who soon after published a textbook on the subject (Reinsch, 1900).
- 4 The London School of Economics developed a department of International Relations in the early 1920s. The Graduate Institute of International Studies in Geneva was established in 1927 to educate diplomats for the League of Nations, whose headquarters were located there.
- 5 In 1931 Farrel Symons gleaned the course catalogues for 465 American colleges and universities in search of course offerings in international affairs. He found 573 courses in all – 234 courses on International Law, 75 on International Organization, and 264 on International Relations broadly defined (Symons, 1931).
- 6 The minutes from the ISC meetings are rich source material for the content and development of IR. The annual lists of participants is a who’s who of IR scholars in the interwar years. Among the names we find on these lists are Stanley H. Bailey, Charles A. Beard, Edward H. Carr, John F. Dulles, Ernest Haas, Hans Kelsen, Christian L. Lange, Hersch Lauterpacht, Charles A.W. Manning, Paul Mantoux, David Mitrany, Philip J. Noël-Baker, Pitman Potter, Georg Schwarzenberger, James Shotwell, Nicholas J. Spykman, Arnold Toynbee, Arnold Wolfers, Quincy Wright, and Alfred Zimmern.
- 7 A new interest in disciplinary history followed the end of the Cold War and produced a thorough discussion of the writings of early twentieth-century IR authors. Pioneering contributors have in particular re-examined and problematized the so-called Idealist school. See Long and Wilson (1995); Osiander (1998); Ashworth (2002), and many others. See also Knutsen (2016, esp. Chapter 8, for a contextual discussion of interwar IR).
- 8 The literature is vast; see Chakrabarty (2007) and Shani (2008).
- 9 There is also a more radical alternative than the establishment of an alternative, post-Western IR, e.g., a call for the destruction of IR. In “order to save it” – or in order, as Foucault (1973: 342) once put it, to unfold a new space “in which it is once more possible to think.” It is hard to know on the face of it whether this is nonsense of a nihilistic or of a hopelessly romantic kind.
- 10 Before brushing Reinsch off, it is worth recalling that his argument did not hinge on Western arrogance but involved concepts like identity and localism. If non-Western populations were to adopt Western-style institutions and norms of behavior, this

would involve nothing less than a program of complete cultural assimilation. Such programs have “in practice proved unsuccessful and at times even disastrous” continued Reinsch (quoted in Hobson, 2012: 122). Western reformers will get nowhere unless they first study the moral character of the natives, he concluded. Almost identical conclusions have been reached one century later by scholars who have examined the costly Western failures to build new regimes in Afghanistan and Iraq.

4 The role of theory for knowledge creation in IR

A sociable pluralist discussion

Annette Freyberg-Inan

Introduction

This chapter serves two purposes. First, it complements discussion in the previous chapters, which has revolved around the meanings of “science,” competition between different philosophies of science, and ways in which we might deal with such contention in the service of creating useful insight for IR, with a specific focus on the role of theory for knowledge creation. As empirical research is, in essence, always about comparing our hunches to data, scientific progress through empirical research depends vitally on the quality of our hunches, that is, on the manner in which we construct and apply theory. I plead for a view of the role of theory that is compatible with this volume’s guiding notion of sociable pluralism – a view, I hold, that is in principle compatible with the entire range of philosophies of science laid out by Patrick Jackson (Chapter 1), yet aligns most closely with Colin Wight’s (Chapter 2) critical realist take on the requirements for theoretical validity and utility. The second purpose of this chapter is to serve as a link between the two halves of this volume by drawing together arguments from all other chapters to present a systematic and coherent view on the role of theory to facilitate the ongoing discussion between different takes on philosophy of science in IR as represented in this book. In this way, this chapter also sets the stage for moving on to the following case study, carried out over several chapters, of the democratic peace.

I intentionally leave open the question of how “progress” in our field should be defined.¹ It can mean cumulation of knowledge, to the extent that we can reach agreement that knowledge has cumulated. Yet progress can also legitimately be defined differently. In any instance, I hold that there can be no objective criteria to measure progress, since as IR scholars we are engaged in an inherently social and political activity. Our standards are what we make them and, while we have a minimum of openness, will always be contested.² Thence, Torbjørn Knutsen’s argument in Chapter 3 reveals a paradox: quality social science depends on open societies, yet it is precisely to the extent that our societies are free that we will never agree on how well we are doing our work. It is true that, as Fred Chernoff argues in Chapter 5, using the same criteria of evaluation could be a key driver of progress. Yet we cannot agree on such criteria, and

very probably should not.³ It is easier, however, to reach agreement on the role of theory in the process and that, then, is my way into this discussion.

At least since Martin Wight complained more than half a century ago that “International theory, or what there is of it, is scattered, unsystematic, and mostly inaccessible to the layman” (Wight, 1960), we have suspected that IR may have a problem with the use of theory in the service of creating useful knowledge. Wight blamed this weakness of theory about the International, first, on a preoccupation of those (few) who are inclined to think theoretically about politics with the domain of state sovereignty (as opposed to the domain of IR, which lies mostly beyond the realm of state sovereignty). He also blamed it, second, on the general gloominess of the international realm, which seems stuck in inescapable normatively undesirable patterns. Quite paradoxically, the very fact that strong patterns can be observed in the international realm, which should make that domain particularly amenable to successful theorizing, reduces our motivation to do so, according to Wight.⁴ This relates directly to an important point raised by Piki Ish-Shalom in Chapter 8 of this volume: our theories (and the concepts of which they are comprised) always reflect our normative commitments (see also Bevir and Kadar, 2008). It is thence unsurprising if scholars fail to develop and use theories which would seem to represent realities unpalatable to them. Thence, also, arise the constitutive silences in our field of which Laura Sjoberg speaks in Chapter 9. Theoretical endeavors on the margins of the discipline have a hard time being heard in the mainstream; this is also because those who “run the show” in international politics and in IR would not like to see the world, and their roles within it, in the ways proposed from the margins. These are important caveats which I place front and center, as they determine to a large extent the substance of the theories which will be constructed and applied and therefore define our field and its development, as sketched by Knutsen in Chapter 3. And still, in spite of the power structures and mechanisms of socialization in place in the praxis of international politics as well as the scholarship of IR, Martin Wight’s basic observation that our field fails to display theoretical integration remains valid. The question is: How should we feel about that?

Calls for theoretical integration should always ring alarm bells, as they represent a search for closure at odds with the determined openness and willingness to embrace failure of a critical scholar (see Sjoberg, Chapter 9). Yet the anxiety raised by Wight does connect with at least two aspects of IR scholarship that may indeed be viewed as problematic. They will be introduced here and explained more fully later in the chapter. The first is the strong drive which has characterized and shaped our discipline to develop and defend “grand” theories which are both abstract and general enough to lay claim to some form of disciplinary unification. This is a result of Wightian anxiety which never quite seems to leave us. Kenneth Waltz’s assertion, made in 1975, that balance-of-power (b-o-p) theory is the “only” theory of international relations can be better understood before this backdrop (Waltz, 1975). Theory, in Waltz’s view, requires abstraction from context and should point to general patterns characterizing the

domain (and level of analysis) examined – in our case that of international politics. While there are other attempts at generalization about international political patterns than b-o-p, they are in his view little more than vague conceptualizations, lacking clear and stable specifications of the relevant variables. Waltz's influential judgment of the landscape of IR theory thus clearly shows twin biases in favor of maximum generalization and variable analysis combined. As much as more novel approaches since the constructivist turn have sought to delegitimize such grand explanatory theorizing, we seem not to have really gotten over this drive, even as it has not led to unification. Instead we remain suspended in rivalry between different grand theories which often appear incommensurable and whose applicability to real-world problems is, due to their abstract and general nature, highly precarious.

The second, and connected, problem invoked by Wight's critique is the "cacophony" mentioned in this volume's introduction. A few years ago Snidal and Wendt (2009) disputed Wight largely on his own terms, coming to the conclusion that much of the malaise he diagnosed a half century ago has been overcome, as International Theory has rapidly developed alongside greater faith in the possibility of progress in world politics – thus ameliorating both problems he identified as causing the dearth of theory in our field. However, Snidal and Wendt also pointed out that "different theoretical communities are not engaging each other in ways that could be mutually productive." Yet more recently, a much discussed special issue of the *European Journal of International Relations* on "The end of International Relations theory?" also stressed the "coexistence and competition between an ever-greater number of theories" and advocates a very careful move toward greater coherence termed "integrative pluralism" (Dunne *et al.*, 2013).⁵ Knutsen warns us in Chapter 3 that we have so far only escaped a situation of endless accumulation of theories and "theoretical overload" by allowing old theories to quietly fade away through an "arrogance of the present" and a tendency to focus on current events.

Clearly we face a time when the nature and quality of theorizing in IR, as well as the extent to which the field's theories can and should provide it with some form of coherent disciplinary identity, are very much under discussion. At stake is ultimately the question which underlies this volume: How do we achieve "progress," whatever that means? Which ways of constructing and applying theory work for advancing our field? The main fault line in this discussion tends to run between those who seek greater theoretical integration (in this book: Chernoff, Hayes and James, Harrison) and those who are skeptical of such moves (in this book: Jackson, Ish-Shalom, Sjöberg).⁶ But there are a great many other disagreements that divide either camp or run across them. I begin this chapter by considering the basic questions of what theory is in the first place and what its functions are for social scientific research. I then return to the specifics of our field and elaborate on the particular challenges faced here with respect to the use of theory. The chapter ends with a discussion of how, in the service of sociable pluralism, we might define theoretical validity and utility in ways that can be compatible with a broad range of philosophy of science positions. Along

the way, I will tie together different elements of the arguments made by my colleagues in the other chapters of this book.

What is theory?

To begin from what should be an uncontroversial definition, a theory is a set of connected propositions about something in the world which we want to describe, explain, or make sense of in some fashion. Typically we think of theory as being more complicated than one single proposition, yet it need not be more complex than a set of two linked propositions. For example, the statement “Russia’s intervention in Crimea is an act of imperialist aggression and is motivated by Putin’s quest for regional dominance” could already qualify as a theory, as it contains a descriptive (more precisely a categorizing) and an explanatory proposition, which are linked to make sense of the same empirical event. Theories do not need to be general – they may pertain to singular cases. They do, however, have to entail a minimum level of abstraction. A “pure,” that is, a non-conceptualizing description of empirical occurrences, if such a thing were possible, would not be a theory, no matter how complex.

Concepts are the key building blocks of propositions and, by extension, of theories. Together with any regularities the theory may propose, they are what does the work of abstraction (Gerring, 2001). The moment we put our observations and judgments into words in ways that go beyond pure recounting of the form “John said this and then Bob said that” – Guzzini’s (2000) idea of a “brute fact” – we begin to conceptualize.

This is evident in statements which label and categorize, but even in statements of a more openly descriptive nature. While a gun, for example, is a material object, the concept of a gun is an abstraction which we can put to very different uses. For example, when we speak of Crimeans going to the polls to vote on secession “at gunpoint,” we may envision this literally being the case, but really we are employing the gun as a metaphor which invokes much theoretical baggage and transports, *inter alia*, our normative views on democratic self-determination (see Ish-Shalom, Chapter 8). When we label or categorize, we abstract by subsuming the specific case or cases with which we are concerned under the label or category. The Crimean invasion becomes *a case of* imperial aggression. One might call this theoretical generalization, but at the very least it is abstraction from the empirical givens of the case (Gerring, 2006). The researcher’s interpretation is added.

Explanatory theories abstract in a different manner. In their quest to identify effects to a given cause (*x*-centered) or causes for a given effect (*y*-centered), or to achieve dis- or confirmation of a proposed cause-effect relationship (*x/y*-centered),⁷ they relate the specifics of the case(s) under investigation to more general patterns. The case again becomes *a case of*, e.g., a case of the drive for hegemony causing military conflict, but what is being abstracted here is not a feature of the case (or the case as a whole) but a more general pattern which the case is said to experience (Gerring, 2006). In this sense explanatory research can

be said to always go “beyond the case” not only theoretically but, at least theoretically, also empirically, even if the causal relationship theorized has materialized only in one single context. This is true for all causal theories except those that are purely constitutive, in which cause and effect are co-emergent out of one and the same set of properties (e.g., “this is a free country by virtue of being a democracy”). Except for these latter types of claims, whose status as genuinely causal may well be disputed, causal claims thus always carry an element of empirical generalization. Predictive theories, which are always at least implicitly based on such claims, of course do as well.

The different types of theories can be seen to build on one another. This can be seen most easily when considering the stylized form of a hypothesis “an increase in x leads to an increase in y .” To assess the validity of this hypothesis, we must first learn about the behavior of both x and y .⁸ Description of variation in the independent and dependent variable is part of any explanation of the link between them. Explanation in turn forms the foundation for prediction, in all cases except the (unsatisfactory) one when unexplained trends are simply predicted to continue and when therefore description is what forms the foundation for prediction.

I have not yet mentioned normative theory. If one considers the types of theories as building upon one another, descriptive theory is foundational, as we have seen, explanatory theory is considered most valuable by positivists, but prescriptive, or normative, theory constitutes the “highest” form of theorizing. That is because it builds on insights derived from both descriptive and explanatory (and predictive) theory to justify its normative claims. For example, a normative theory arguing that NATO should not respond militarily to the Crimean invasion would likely be based on the observation of patterns of conflict escalation in comparable circumstances. As Colin Wight explains in Chapter 2, social science researchers always mix rationalism and empiricism; this is true in both positive and normative theorizing. However, philosophies of science take varying views on just how different positive and normative theorizing are. While those more closely aligned with positivism emphasize that normative theory argues more strongly from principle and less with reference to evidence and therefore contributes something qualitatively different to our disciplinary discussions than does empirical research, non-positivist approaches, as represented by Ish-Shalom and Sjöberg in Chapters 8 and 9, draw attention to the normative commitments inherent in *empirical* research. They thereby work to undermine the distinction between positive and normative theory. In the words of Ish-Shalom, they reveal a constitutive relation between the explanatory and the normative.

What is the role of theory?

Bear with me while I start from a still useful cliché. On one level, theories function as lenses upon the world we perceive, the world of our evidence, the data we use to come to our judgments. A theoretical perspective will “tint” this world, as colored glasses will provide a tint to the landscape we observe. Already in

this very basic sense, different theories look at the world, they “see” things differently. But there is more to it: the inescapable function of theorizing is that of necessary simplification for the sake of being able to interact with the world. Perceiving without conceptualization and theorizing would be like being drowning in perceptions, unable to order them, to process them, or to come to any conclusions about how to judge or react to them. Theory is in this sense an act of self-assertion of the human subject in the world, the act of theorizing an act of differentiation from the surrounding world, and as such the very seed of agency. However, at the same time experiencing theoretically changes us. It is more akin to Zaphod Beeblebrox’s Super-Chromatic Peril Sensitive Sunglasses than to any ordinary ones,⁹ since the world also “speaks” to us through our theories, and we cannot but listen. This is important to recognize, not least to be able to comprehend our own and others’ theoretical likes and dislikes, which may often not be motivated by judgments of scientific validity or personal utility but by the very different but equally legitimate identity-based question “what would thinking this way say about and do to me?”

Methodologically speaking, theorizing provides us with the descriptive, explanatory, prescriptive, or normative hunches which we can then, in empirical research, compare to the world of evidence. It does so by orienting us among limitless possibilities for where to focus toward an emphasis on some specific features, patterns, mechanisms, or values at the expense of others. In this sense theories can also be claimed to “model” reality. As Colin Wight explains in Chapter 2, theories never replicate reality. They are abstractions. But they do have a relationship with the reality they represent that needs to make sense. Traditionally, descriptive models are considered successful to the extent that they allow us to “recognize” the theory’s empirical referent(s), like a model airplane, which does not need to have windows or wheels, even though they are equally real, but which would be considered a failure without an elongated body or wings. The value of explanatory models traditionally tends to be judged by their predictive capacity and/or their ability to provide plausible explanations for observed regularities. The value of normative models tends to be judged by the rhetorical force of the argument, the validity of its empirical claims, and the match between the norms prioritized and those of the person doing the judging.

All empirical research requires theorizing, at the very least during the stage when we have to make sense of the data observed. Yet, it is vitally important not to relegate theory to the status of a handmaiden of empirical research. Once again, we can observe a tension between broadly speaking positivist and non-positivist approaches on this issue: positivist approaches tend to confine the realm of legitimate theorizing to those activities that promise immediate pay-off for furthering empirical research by developing testable propositions. Non-positivist approaches, by contrast, tend to value theoretical interventions which subvert mainstream, positivist ideas about how to judge theoretical progress that are tied to empirical testing.¹⁰ A good example is Gilles Deleuze’s concept of “assemblage”: a performative concept he created in part to reject the very idea that a concept should be matched to a clear empirical referent and thereby

become able to function in propositions susceptible to even the very softest form of positivist empirical assessment. While this poses serious challenges for empirical research,¹¹ it is all still part of legitimate *theorizing* in our field: the attempt to conceptualize what is going on, why it is going on, and how we should think and feel about it. Baudrillard's stance of ambivalence, as discussed by Sjoberg in Chapter 9 of this volume, is another example of such an intervention that seeks to subvert an empiricist bias in judging the value of theory.

The special challenges of theorizing in IR

In the field of IR we can observe a twin and to some extent self-contradictory bias in favor of hypothesis-testing research and grand theorizing. First, among those who follow, broadly speaking, the KKV approach to judging the value of different types of research (King *et al.*, 1994),¹² the view is at least implicitly current that "good" theory, i.e., the kind that supports valuable research, is explanatory theory and, moreover, explanatory theory of the x/y-centered type. This bias in favor of the hypothesis-testing type of research, which is traditionally though not logically aligned with a positivist philosophy of science, has had the effect of narrowing our disciplinary vision of theory and its role in IR. It is partly explained by the earlier observation that explanatory theory tends to impart empirical generalization – generalization of course being a key criterion for useful social science from a mainstream point of view.

Another part of the explanation is to be found in an instrumentalist view of social science impatient to get from scientific discovery to practical advice: learning about the mechanics of "what makes what happen" (and under which circumstances) delivers more immediate pay-off for the design of interventions than the groundwork of descriptive observation or normative reflection. This impatience also helps explain the bias in favor of hypothesis-testing as opposed to more exploratory types of explanatory research. It in effect relegates all research that must come before the identification of a plausible hypothesis to pre-scientific status, so that scientists can get right on with the business of testing whether a suggested fully developed explanation stands up to scrutiny.

But plausible fully developed explanations do not fall from the sky. Even if we do consider explanatory empirical research the highest form of social science, it would be patently silly to devalue descriptive research, on which it must necessarily be based, or the at least partially prescriptive research which seeks to develop its implications. And those necessarily complementary types of research have their own bodies of theory. The hypothesis-testing bias enshrined in mainstream notions of what IR theory should, at the operative level, be about, ought to be rejected.

A second bias with respect to the role of theory which characterizes IR is connected to the dominance of grand theories, or even rival ontologies, which dominate our field and even serve to define it. As a discipline, IR is remarkable for being defined not primarily by its subject matter or key empirical regularities it observes and explains, but rather by its evolving body of grand theories and the

“great debates” among them. When we produce IR graduates we tolerate that some of them learn a lot about war and peace, others about global finance, and yet others about environmental INGOs, but we expect them all to learn about realism, liberalism, and constructivism.¹³ Perhaps we need this canon to rally around, and its resulting reproduction, as a disciplining device, given that our empirical subject matters seem, to many of us at least, in rather rapid flux, and given that they are studied by other fields as well. After all, while drone warfare or credit default swaps are quite new, realism seems to have been around forever without ever losing all of its plausibility as an interpretive lens. Moreover, as Dunne, Hansen, and Wight also point out, “given the disciplinary competition that now exists in relation to explaining and understanding global social forces, International Relations may find resilience because it has become theory-led, theory-literate and theory-concerned” (Dunne *et al.*, 2013). While they did not have grand theories (exclusively) in mind when making this statement, inasmuch as its grand theories distinguish IR most completely from other fields, they may bear a good part of the resilience here mentioned.

But there is a downside to the veneration of our grand old (and new grand) theories: empirical researchers in IR forever live with the problem that those theories are simply too abstract, too far away from the guns, handshakes, and green papers which make up the real world of international politics. While these theories, being in fact ontologies of IR or even the social world more generally, can make post hoc sense of very nearly everything, they are not amenable to actual empirical testing. The best we can do is to derive more concrete expectations from them – a process highly susceptible to criticism – and then set about testing those. Since these implications will never exhaust the theory from which they are derived, the theory itself remains irrefutable. In this manner, our grand theories have preserved a status outside IR as social science and form, as it were, a philosophical protective belt around our discipline. We will never agree on embracing any one of those grand theories at the expense of the others. But we agree on accepting them collectively as defining our field. As Knutsen points out in Chapter 3, theories have a way of accumulating rather than truly replacing each other.

Actual empirical research in IR remains largely disconnected from these grand theories, because it cannot help it. It either engages in exploring or assessing implications that are, more or less loosely, connected with the grand theories. This may or may not take the form of “mid-level” theorizing as called for by e.g., George and Bennett (2005), who propose to close the gap between grand theoretical abstraction and empirical messiness by developing theory at the “in between.”¹⁴ Or it proceeds in ways which do not connect to the grand theories at all but instead conceptualize in different ways. While there is nothing wrong in principle with such more inductive and/or innovative approaches and the pluralism they foster, this does pose a problem for the cumulation of knowledge or insight. Large bodies of empirical work have by now coalesced around new conceptualizations like securitization or governance, but it is often not clear how they relate to the discipline’s great debates or what implications they have for the struggle between grand theories that keeps going on in the background.

So to what does this add up? A triple-layered view of our discipline emerges: at the most abstract level of grand theorizing we see a remarkable resilience of broad rival ontologies that reflect a drive for (but never achieve) theoretical unification and, for all their mutual incompatibility, provide us with a sense of disciplinary identity. Of course those grand theories are not entirely stable. Yet, ultimately, the field remains characterized, at this level, by a handful of diachronically identifiable and largely incommensurable worldviews. At the mid-level of conceptualization without grand theoretical pretension openings emerge, around which new cliques form. Broadly fitting Waltz's criticism of "vague conceptualizations," they rally around broader labels like "International Political Sociology," but often do not develop full-fledged empirical theories and typically do not contest the grand theories at the level of empirical research. This last level, instead, remains dominated by a positivist mainstream which is biased in favor of hypothesis-testing research, maintains precarious connections to the grand theoretical debates, and into which the useful innovations made at the mid-level have made few inroads as of yet. In this manner the theoretical makeup of our field has contributed to the epistemological and methodological "cacophony" which we have observed in the introduction to this volume. In the last section of this chapter I want to build on the preceding observations to suggest a take on judging the validity and utility of our efforts at theorizing that draws on the contributions of all the volume's authors to represent a sociable pluralist position on the role of theory for creating insight in IR.

Judging theoretical validity and utility: a sociable pluralist take

In conclusion I use the terms "validity" and "utility" to structure a discussion of how to judge the value of our theoretical endeavors in a manner which, in the spirit of sociable pluralism, is open to various philosophy of science positions. The choice of terms is important. Validity and utility invoke different criteria, and while the former might be claimed to carry a positivist bias, the latter label is more inclusive of other philosophies of science. I begin with a discussion of criteria for theoretical validity as presented earlier in this volume by Colin Wight (Chapter 2), although I take the liberty of rearranging them for my own purposes.

Wight begins from the observation that the propositions and theories with which we work can never really be known to be "true" or "correct" in relation to reality. Theories after all never replicate reality but always abstract from it. However, as Wight explains, they do have a relationship with the reality they represent that needs to make sense. This "sense" is typically spoken of in terms of validity. We might put this differently by saying that we must recognize our perceptions of reality in the theory to find the theory acceptable. This is the first of five validity criteria.

In addition, theories are constructed with specific interests in mind, and we then, second, judge them by how well they enable us to achieve those interests.

In Wight's words, if a scientific theory provides a reliable guide to action, then we can say it is valid (Chapter 2). At this point, Wight makes the plea for a realist ontology, which is implicitly shared by almost all authors in this volume:¹⁵ Realism as the idea that there is an empirical reality beyond our perceptions and interpretations of it pushes us always to confront our conjectures with evidence. This, Wight holds, protects us against dogmatism and sustains the logic of science, or what we refer to in the conclusion to this volume as the logics of discovery and confirmation. In this sense, it "provides the conditions of possibility for why a theory is a reliable guide to action" (Chapter 2 in this volume). This resonates also with Knutsen's claim (Chapter 3 in this volume) that to the extent that IR is significantly shaped by international events, it would make sense to pay more attention to the events that shape it and less to the accumulating variety of specialized theories that are shaped by them. An additional and important argument in favor of realism is that the search for "truth" helps insulate science to some extent from other social pressures, such as those posed by political or career incentives. All in all, we can distill from these arguments in favor of a realist ontology the second element of our sociable pluralist strategy as laid out in the conclusion: the need for soft positivism in the *logic of confirmation*, based on the idea of a reality which is at least partly external to us and the use of inter-subjective standards for confronting our hunches with what we can glean of that reality.¹⁶

Wight's third criterion for theoretical validity is constituted by the validity of the background assumptions about the nature of the reality being theorized. Fourth, he holds that various claims made within the same theory need to make sense in relation to one another, and that the theory has to make sense in relation to alternative theories of the same, or overlapping, object domains – to the extent that they all have been judged to possess validity, that is. Lastly, Wight points out, fifth, that theories are validated by the assent of the scientific community. In Chapter 3 by Knutsen we have learned about how this has happened for the field of IR, and in Chapter 7 by Harrison we will find an illustration in the evolution of the DP research program. However, Harrison's chapter also reveals an important drawback to this fifth validity criterion: "decided opinion" can become stultifying. Harrison observes, on the basis of an analysis of the trajectory of the democratic peace research program, that scientific progress is not possible to sustain over the longer term without questioning previous consensus and taking risks by means of theoretical leaps, and that failure to do so can lead to stagnation, excessive incrementalism, and growing irrelevance.

This leads us directly into a discussion of the second evaluation criterion I foreground here, that of utility or usefulness. As explained in its Introduction, this volume is motivated by the recognition that as social scientists we are called upon to justify our use of public resources, and by the conviction that reflecting on philosophy of science can help us figure out how we can both do and sell our work better. It is not difficult to recognize that we are confronted with powerful pressures to think of our own work along instrumentalist and pragmatist lines, in order to maximize its visible payoff for political elites. However, this does not

mean that we should simplify and constrain our theorizing to create “how to” manuals. On the contrary, in this volume we have already heard several arguments connecting instrumentalism and pragmatism instead to a need for theoretical openness and an embrace of complexity. A clear example is Chapter 6 by Hayes and James. The authors take a pragmatic approach to theorizing by engaging in analytic eclecticism, which focuses on solving problems “as they are understood by political actors” and without adherence to the “scholarly conventions or theoretical boundaries” that guide and constrict paradigm-based research. In addition, they embrace complexity in causal explanation through the use of systemism, which allows for linkages operating at macro- and micro-levels, along with back-and-forth between them. The pay-off of such an approach, as demonstrated here, is a better understanding of how we have achieved what we perceive as progress in the evolution of the democratic peace research program.

The volume as a whole echoes this call for *openness in the logic of discovery*, which is the first element of our sociable pluralist strategy as laid out in the conclusion. Jackson, in Chapter 1, asks us to rethink the boundaries of IR in such a way as to make more room for ethics, art, and engineering as modes of inquiry – alongside a suitably pluralized notion of science. Sjöberg in Chapter 9 reconceives the ideas of cumulation of knowledge and progress in IR as productive fantasies: social performances which may deliver useful results as long as we are aware that there are no judges other than ourselves. While Wight (Chapter 2), Harrison (Chapter 7), and Chernoff (Chapter 5) do not share her radical non-foundationalism, they do not disagree with the sociology of science observation that the scientific community can indeed function in the ways Sjöberg describes.

Last but not least, theoretical usefulness also depends on the recognition of the roles played by normative commitments in scientific research. Ish-Shalom reminds us in Chapter 8 that theorists must reflexively and critically engage with the moral commitments that inform their theoretical work and keep an eye on the real-world ramifications of their theories. Sjöberg calls upon us to probe the constitutive silences that shape our research programs and determine what we do and do not end up saying.

To return then, lastly, to the questions raised at the outset, there is no need for theoretical integration in IR. On the contrary, theoretical pluralism and openness – alongside an embrace of real-life complexity which leads away from the grand theoretical hang-ups discussed earlier – are productive for our field. When it comes to approaching a consensus on how to achieve progress in spite of theoretical openness, the views on the criteria of validity and utility presented here might be a first cut, if we are so inclined.

Notes

- 1 I also do not embark on a discussion of the label of “science.” For a useful discussion see Jackson in Chapter 1 of this volume.
- 2 On these points see also Sjöberg in Chapter 9 of this volume. Here we also find a useful discussion of how by giving up pretensions to knowledge cumulation, or

re-conceptualizing them as productive fantasy, it becomes possible to see – and learn – IR differently.

- 3 Chernoff also holds that clarifying the precise question to be answered and the contrast class are additional important elements for achieving progress through empirical research. With these points I agree.
- 4 This interpretation of Wight is debatable. As Weber (1998) has pointed out, “interpreting Wight’s essay has become something of a cottage industry.” I do not enter these debates here.
- 5 See also “The End of IR Theory” Special Issue Symposium on The Duck of Minerva blog at www.whiteoliphant.com/duckofminerva/tag/ejir-special-issue-symposium.
- 6 Wight and Knutsen are sitting on the fence on this question.
- 7 These are the simple types of explanatory propositions that can be at the heart of causal theories. In practice, much explanatory research does something rather more complicated, like combine any of the above types with the search for limiting, activating, or threshold conditions for causal relations to take effect. For a discussion of complex causality see Wight (Chapter 2) and for an illustration using the DP research program see Hayes and James (Chapter 6).
- 8 A hypothesis is of course only one very specific form of a proposition. My use of the example here by no means reflects a preference for propositions of this form over others.
- 9 Douglas Adams came up with the idea of sunglasses designed to instill in the wearer a relaxed attitude in the face of danger. The lenses turn solid black at the first sign of trouble, thus preventing visual perception of anything alarming. The sunglasses appear in episode 3 of the TV series *The Hitchhiker’s Guide to the Galaxy* and in chapters 5 and 6 of the novel *The Restaurant at the End of the Universe*.
- 10 One explanation for why the mainstream is dominated by positivist philosophy of science is offered by Jackson in Chapter 1 of this volume.
- 11 See Hammersley (2008) for a powerful critique of what he calls “Dadaist” research that undermines its own real-world relevance.
- 12 Not only in North America but also in Europe the “KKV take” can be considered paradigmatic for the epistemological and methodological mainstream in empirical research in IR.
- 13 Constructivism is not a substantive grand theory of IR itself but a meta-theoretical approach with both ontological and epistemological characteristics that can and does align with different substantive theories.
- 14 See also Adcock and Collier (2001), who suggest beginning empirical research from what they call systematized, more concrete concepts as opposed to excessively general background concepts.
- 15 Sjöberg in Chapter 9 is a clear exception. Ish-Shalom in Chapter 8 might be another.
- 16 Here conventionalism becomes an ally of realism, in the sense that intersubjectively developed standards are applied in research based on a realist ontology. See Wight in Chapter 2.



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Part II

Evaluating progress in democratic peace research – an illustrative case study



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5 Bounded pluralism and explanatory progress in international relations

What we can learn from the democratic peace debate

Fred Chernoff

Introduction

The study of IR has the potential for acquisition and growth of genuine knowledge, at least in some issue-areas. Progress of this type requires the resolution of disagreements between disputing schools of thought. Disagreements often stem from differences over empirical evidence and theoretical principles; but they can arise also at the meta-theoretical level – over selecting the right criteria of theory choice or the proper form of a social science explanation. Only when the locus of the differences are identified can the disputants then focus debate on the issues that divide them, engage opponents' arguments rather than talk past one another, and work toward showing more persuasively that their preferred conclusion is superior. The discussion that follows argues intellectual progress in finding best explanations requires both careful attention to the criteria of theory-choice (H-Conv) and unfettered criticism of methodological principles (H-Plur).

The chapter builds on three premises that I have supported in other published works. *Premise 1. Many forms of inquiry are legitimate.* IR is an interdisciplinary field and, for that reason, there is a range of *types* of question that scholars in the field ask, including empirical, meaning-interpretive, causal, theoretical-explanatory, moral, etc. I have argued in *The Power of International Theory* (2005) and elsewhere (e.g., 2007b) that the diversity of questions justifies a range of legitimate and appropriate methods of argument, including empirical, interpretivist-constructivist, moral-evaluative, and philosophico-meta-theoretical. The appropriate method or combination of methods depends on the precise nature of the question at issue. It might be added that, because *The Power of International Theory* was written in a period in which empiricist theorizing was the subject of a sustained interpretivist-constructivist assault, often supposedly bolstered by meta-theoretical principles of scientific or critical realism (Wendt, 1987, 1992, 1999; Patomäki and Wight, 2000a), much of the book shows how those foundational criticisms miss their mark and how empirical methods, when applied to a particular range of IR questions, are at least as legitimate as constructivist or other approaches.

Premise 2. Sometimes scholars are wrong. A driving concern motivating my work in the past 15 years is the question of whether we *learn* anything in studying IR. That is, do the books and articles we read in IR provide us with *knowledge*, given any reasonable definition of the term? Because philosophers continue to debate the proper definition of the term, all that is assumed here is the following: if researchers S and S' have access to the same evidence, are investigating the same question in the same context, understand the question and all its terms in the same way, and S knows that P is true, then if S' denies that P, S' is wrong. This would hold on almost any theory of knowledge.¹

IR of course has scholars who disagree in ways in which neither one is wrong in any straightforward sense; they often present partial results (whose partialness they may underappreciate) and may be explaining different aspects of the same phenomenon or process. However, in studying IR one finds very few instances of scholars acknowledging their own blunders.² If we view S as knowing that P and S' as denying that P but we are hesitant to call S' *wrong*, then it would seem that we cannot refer to S, or for that matter to anyone in IR, as *right*; hence we could not say that IR enables scholars to gain knowledge. This leads to fundamental questions: Do scholars in our field ever have an obligation to admit they have been wrong? (On this point, see Tetlock, 2005.) Is the potential to be wrong one of the risks we run, or in IR are scholars always legitimately permitted to avoid revising their earlier positions regardless of other scholars' well-founded but contradictory findings?

Previously I have tried to show that scholars in empirical study do run the risk of being wrong, and thus to support the view that IR scholars are indeed capable of epistemic progress, of learning new facts and, given all the current evidence, of identifying best available explanations or best interpretations. Negative answers to the questions in the previous paragraph entail that it must be impossible to find resolutions to some IR debates. And, since so many debates seem to be moving nowhere, to advance knowledge we must understand how some debates get resolved.³

As I have argued elsewhere (2004), the fact that running the risk of being wrong entails that what philosophers of science regard as scientific progress is possible in IR – and has in fact occurred in the case of the DP debate. It is beyond the responsibility of IR authors, and beyond the scope of IR publications, to pick out the single correct account of “science” and “progress” found in the vast literature in the philosophy of science. Thus the strategy of that article was to outline five of the most widely accepted accounts of “science” and their attendant criteria of “progress,” and to show that the evolution of the debate on democratic peace meets the criteria, no matter which of the accounts one prefers.

Premise 3. While many things may “go,” it is not the case that “anything goes.” With respect to the methodological pluralism noted in premise 1 above, the view I defend is far from an “anything goes” form of pluralism. The philosophical and meta-theoretical debates can both reveal flaws and identify strengths in various methods of study. When cogent and persuasive arguments reveal such flaws, at least as those methods are applied to certain types of question, it is not acceptable

to apply to them in those contexts. This is taken up again in section 3, where the position advocated here is contrasted with the view Jackson presents in Chapter 1 of this volume.

These eight sections of this chapter argue, respectively, as follows:

- 1 In IR there is a wide range of concepts of “explanation” but this need not prevent progress.
- 2 There are many conceptions of “scientific explanation” in the natural sciences but disagreements among philosophers on the concept of explanation have not prevented scientists and philosophers from agreeing on which theories count as progress over their older rivals.
- 3 Some authors argue that we must choose between (a) allowing that different explanatory traditions develop separately and studies within those traditions are incommensurable; or (b) advocating a single framework or model of social inquiry. But this chapter argues (c) that a *bounded pluralism* is preferable. On this view many types of method are appropriate generally speaking but for any particular question, the appropriate methods are bounded as a result of meta-theoretical debate that reveals strengths and weaknesses of methodological candidates.
- 4 Natural and social scientists learn how to explain by means of exemplar great works in their fields (i.e., one form of convergence).
- 5 In the democratic peace (DP) debate, a rare near-consensus has emerged over the truth of the dyadic hypothesis and liberalism’s ability to explain it; this has emerged out of the wide-ranging realist criticisms in the 1990s.
- 6 The similarity of competing schools’ usages of criteria of evaluation is one key that enables intellectual progress in explanatory debates; this is evident in the examination of the debate over how to explain the dyadic hypothesis.
- 7 Clarifying the precise question to be answered, including the contrast-class, is a major element of progress in explanatory debates.
- 8 Thus, the paper draws two conclusions. One is that examination of the largely quantitative DP debate shows how the key to epistemically progressive research is authors’ ability to interact directly with one another’s theories, which requires that they carefully specify both precise questions and criteria of evaluation. Thus the chapter supports the central hypothesis of convergence and growth of knowledge, *H-Conv: when debates exhibit greater overlap of criteria of theory-choice they are more likely to approach consensus on explanatory answers – and when there is divergent reliance on criteria, they are unlikely to approach consensus*. The bounded pluralism supported here holds that *H-Plur inquiry in IR may proceed according to a range of legitimate methods, where the nature of the question at issue affects the choice of the most appropriate methods and where all methodological principles are truly public in that they are open to meta-theoretical criticism from all reason-based arguments*. The chapter recommends that knowledge can be most fruitfully pursued by scholars making explicit the criteria they use.

The sections below support the argument adumbrated in 1–8 by means of an empirical assessment that tracks the contours and character of the vigorous debate about democracies and peace. To add some perspective, it offers a brief contrast with the contemporaneous post-NPT debate over nuclear proliferation and over balance-of-power theories.⁴

IR possesses a wide range of concepts of “explanation”

Naturalism in the philosophy of social science is the idea that inquiry into the social world should be modeled on the study of the natural world. While many IR authors make use of some form of natural science-like explanation, others reject this and argue that explanation of the social world must be an interpretive effort to find the meaning of human behavior. They offer several lines of argument for this position. Consider just two. One is that the social sciences, unlike the natural sciences, study units (e.g., human beings) possessing free will and the ability to choose to act contrary to any generalization that social scientists produce, no matter how universal that pattern of behavior might have been in the past. A second is that human behavior is intentional and by virtue of its nature conveys meaning, which is again fundamentally different from the behavior of the objects that natural scientists study (Winch, 1958). These scholars argue that the study of the social world bears much closer similarity to the study of language than nature.

Both naturalist and interpretivist scholars rely on criteria, usually implicitly, as they develop arguments seeking to show that their own answers are the best. While there is no space to describe in detail the top criteria that philosophers of natural science invoke to promote one explanation over others, such criteria include: simplicity, explanatory unification, range of phenomena explained, empirical adequacy, falsifiability, identification of causal mechanisms, identification of true causes, depth of causes, supportiveness of counterfactuals, the ability to impart understanding, predictive accuracy, comprehensiveness (or the ability to exclude alternative explanations), and methodological conservatism.

Interpretive explanations similarly require that authors make use of various criteria in providing interpretive analyses or explanations. Explanations should offer *consistency* with other usages (in the case of texts) and behavior (in the case of social action). Good interpretive scholarship should emphasize *coherent* interpretations of the behavior of individuals and institutions. Some form of the *simplicity* criterion applies; that is, in the absence of evidence suggesting that other principles are involved, one should invoke the minimum number of explanatory principles or assumptions. For example, if Vladimir Putin mobilizes troops when conflict begins in South Ossetia, and we later learn that he meant to convey Russian power in so doing, then, when we observe him mobilizing in a similar way in Crimea, it is not reasonable to invoke some other explanatory principle (like protecting Russian speakers’ human rights) unless there is some further type of evidence beyond what was available in the South Ossetia case. Bohman (1993) uses a standard method of arguing for the capacities of social science,

given the limits imposed by indeterminacy and circularity, by using what he regards as the most successful social sciences, ethnomethodology, rational choice, and the theory of communicative action.

Divergent concepts in natural science and a range of criteria in naturalist and interpretivist scholarship

While there is a great amount of disagreement over the notion of “scientific explanation,” even in the natural sciences, the dominant idea has been that science provides causal explanation, that is, to explain an event or process is to find its causes. There is still much to debate about how to explain, even if we accept that science explains causally.

At the turn of the last century empiricism came to dominate the philosophy of science: occult qualities and forces were held to be dubious. However, a dilemma arose because at that time revolutions in areas like astronomy, gravitation, quantum theory, and biochemistry proceeded, resulting in theories that postulated that (in principle) unobservable forces, with wondrous observable results. Two broad trends emerged. One solution was the scientific realist claim that the unobservable entities really do exist, and have the same status as things we observe. This position is put forward effectively by Wight in Chapter 2. The other was the empiricist (anti-realist) view that the goal of a theory, or a theoretical explanation, is to help human beings organize knowledge of the observable world, and perhaps to help humans control the world more effectively. They did not regard theories as producing a literal picture of the physical or natural world.

The best known form of empiricist explanation is, of course, the deductive-nomological (d-n) covering law model of Hempel (1942; see also Hempel, 1945 and 1965; and Hempel and Oppenheim, 1948). Following d-n analysis, a good explanation must fit the form of a deductively valid argument with a set of premises comprised of general (natural law) principles and particular conditions. The conclusion of the syllogism is the event or phenomenon to be explained. What an explanation is, the type of thing it is, is an argument – in particular a deductively valid argument. “Truth” and “explanatory power” have different senses in realist and empiricist interpretations. For a scientific realist, a true explanation corresponds with the objective world, while for an empiricist, a good explanation, or even a “true” explanation, orders our experience better than its rival explanations.

Salmon (1984), as a scientific realist, rejected empiricist accounts, arguing that an analysis of scientific explanation must be able to account also for scientific understanding. He held that explanation is related intimately to understanding (in a way often overlooked in IR meta-theory). Even predictive perfection cannot genuinely explain, since one’s knowledge of the first part of the ballistic trajectory of a thrown ball allows one to predict a later stage of the trajectory, but the earlier stage does not cause – and does not provide any explanation of – the subsequent stages. For Salmon, an explanation must describe a causal mechanism. A causal process is a physical process that continuously transmits a local mark.

Some views of “explanation” build on J.L. Austin’s account of language, one aspect of which was the distinction between ordinary description and “speech acts,” the latter being the utterances of sentences that “do things” which are much different from what the utterances of declarative sentences do. Austin distinguished locutionary, illocutionary, and perlocutionary acts. He argued that the usual distinction between statements of fact and those of value was not exhaustive because some utterances of some sentences *constitute actions* that go beyond merely the action of speaking. To say “I promise I will repay the debt” is not merely to talk about the debt but to *create* an obligation. To say “I now pronounce you husband and wife,” in certain, specific, circumstances, is not merely to describe what one is doing but to perform an action and to create a new state of affairs. Such statements are neither factual nor evaluative. (See also Sjöberg’s discussion of performatives as “disciplining” in Chapter 9 of this volume, section 2; cf. Butler, 1990, 1993.)

According to Austin, an utterance can have “locutionary force,” which is its meaning and reference; it can have “perlocutionary force,” which is the set of effects it is intended to have on others; and it can have “illocutionary force,” which is what one is doing with the specific locution in a certain set of circumstances. Statements may not have all three sorts of force. When someone says, “The building is on fire,” there is specific locutionary force relating to the building and its state of combustion. But the statement could be uttered with illocutionary force if it is intended to warn people in the building. In that case, it may have the perlocutionary force of inducing those people to flee. Austin explicitly includes “explain,” along with “criticize,” on his list of illocutionary acts (1962: 160–161). So part of what qualifies a set of propositions as an explanation is the effect (illocutionary force) that the set of propositions has on the intended audience. Thus, the explanation is comprised of (a relationship between) a set of propositions and some aspect or state of the audience.

On another view, an explanation must turn the unfamiliar into something familiar to the (social or natural) scientific community. One’s ability to gain understanding is part and parcel of having one’s curiosity assuaged; while curiosity-reduction is what accomplishes that goal, the goal itself appears to be the change of the psychological state. The Nobel Laureate P.W. Bridgman said: “I believe that examination will show that the essence of explanation consists in reducing a situation to elements with which we are so familiar that we accept them as a matter of course, so our curiosity rests” (1938: 37; see also Dray, 1957). In this case, what will count as a good explanation is again dependent upon the context of knowledge and the beliefs and assumptions held by the investigator or the audience. This is not the case for the standard d-n model. Still others, such as early twentieth-century empiricists, hold a more austere view of what actually constitutes the core content of scientific inquiry, or even science properly conceived. For them, the function of “making the community more comfortable” is purely heuristic and not itself truly a part of science.

Despite a range of contrasting ideas of what an explanation is, and what traits a good explanation should possess, when disputes arise scientists are nevertheless

able to agree on the better theoretical explanation, such as Lavoisier over Phlogiston, Newton over Ptolemy, or Harvey over Galen. While philosophers of natural science have a number of conceptions of “scientific explanation,” this disparity has not prevented them from identifying instances of “progress.”

Incommensurability versus philosophical debate

Some philosophers of natural and social science as well as some IR scholars follow the Kuhnian (1970) claim that scientific disciplinary matrices or paradigms are incommensurable. According to this view, different theoretical explanations are simply different – there is no theory-neutral language of observation with which to conduct “crucial experiments,” and hence movements in science proceed on non-rational, sociological, or historical grounds (see Chernoff, 2006: 27–28, 180–185). Many others over the decades have put forth arguments that are intended to cross paradigms and intellectual traditions and insist that the natural or the social science must follow a single model in order to produce genuine knowledge. There is, though, a third choice, what might be termed a *bounded form of methodological pluralism*.

This view will be advanced on the basis of two propositions. One is that the norms of explanation develop in specific ways in different issue-areas, which is suggested by the parallel with Kuhn’s (1970) view of the history of physical science (discussed in the next section). A work of scholarship that contributes to a particular debate must take into account the criteria of evaluation and the norms of that debate. These can differ from one issue-area to another and even within an issue-area over time, either by evolving gradually or by authors developing an explicit and compelling meta-theoretical argument about norms. The meta-theoretical norms will ideally include specific criteria of theory evaluation. The criteria may be implicit or explicit. It is argued here that the more explicit they are, the more likely progress will occur. The second proposition advanced here is that, in a given research-question area, once the questions have been clearly defined and terms clarified, it is possible to engage in rationally grounded meta-theoretical discourse about the best method of study – even if there is no single method for all categories of research question.

Progress requires some convergence or agreement on the notion of “explanation.” Science is understood to have several key properties, including an inherent capacity for “progress,” where that includes some combination of: cumulation of knowledge from one generation to another; accurate predictive implications of accepted theories; enhanced ability to control one’s environment; and patterns of approach-to-consensus when controversies arise over the choice of the best theories and explanations (Chernoff, 2014: ch. 2).

By way of contrast, in Chapter 1 of this volume and elsewhere (2011), Patrick Jackson discusses the concept of science and claims that only a broad definition is acceptable in IR. Jackson defines the epistemic work that IR can do as science and defines the concept as “systematic, worldly inquiry subject to public criticism” (Chapter 1). The definition is, however, both too broad and too narrow.

The problem of excessive breadth is apparent on the surface but it is not the main concern here. The narrowness of the definition is problematic because it excludes an essential part of legitimate IR discourse, namely meta-theory.⁵ Methodological claims that are part of a particular cell in Jackson's typology are immune from meta-theoretical charges brought by scholars whom Jackson classifies in any of the other three cells. The narrowness problem is visible by noting Jackson's definition of (social) science mandates that science is not only systematic and worldly but also public, yet the gag order on outsiders' criticism violates that definition because members of the other three cells are certainly part of the relevant IR public. In particular, Jackson's position, which excludes much of meta-theory and the philosophy of science, is based on reasoning that is far too weak to support it (see Chernoff, 2013a).⁶

Meta-theory includes debates over, for example, what criteria should be used to choose the best available theory and procedures to resolve disputes over anomalous observations. Such debates are indispensable for the growth of knowledge. As I have argued in various places (2005, 2012), Duhem (1954) is unquestionably right that procedures make up science; however, they may undergo refinement and evolve over time and are not "purely scientific" in the sense that they cannot be inferred solely from empirical observation and deductive logic. But procedures are not arbitrary. Conventions that science accepts are the result of vigorous debate and reasoning, including inductive inference based on successful past practice. Debates over proper ranking of criteria of theory choice have aided the growth of knowledge in the natural sciences. Thus, Jackson's limitation on meta-theory is crippling to the advance of knowledge. There are solid philosophical arguments for preferring one scientific theory over another and we will see that something parallel to this process has occurred in the DP debate.

Issue-areas and socialization into norms of inquiry

One of Kuhn's important contributions to the sociology of science is an account of how scientists learn what constitutes good practice. Scientists undergo education and "professional initiation" that is "unparalleled in most other fields" (1970: 177). In Kuhn's view, "some examples of actual scientific practice – examples which include law, theory, application, and instrumentation together – provide models from which spring particular coherent traditions of scientific research" (1970: 10). Prior to the relatively recent advent of textbooks, scientists were given important works to read that were regarded as examples of high quality work. Kuhn says:

many of the classics of science ... did what textbooks do today. In physics, classics, such as Aristotle's *Physica*, Ptolemy's *Almagest*, Newton's *Principia* and *Optiks* ... and many other works served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners.

(1970: 10)

This seems to be exactly what education in IR is intended to be like.

The most respected works in each problem-area instructed students explicitly about the solutions to specific scientific problems, but also implicitly about the proper way to carry out scientific research. Such classic works provide the sort of “concrete achievement” which Kuhn argues is “a locus of professional commitment, prior to the various concepts, laws, theories, and points of view that may be abstracted from it” (1970: 11). The parallel with IR is a strong one, as many graduate courses in particular subfields similarly avoid textbooks to introduce students to core debates and rely instead on the works of major contemporary authors. Kuhn notes that the classic works contain rules of method, but such rules are typically implicit. Hence it is not always easy to state precisely what those rules are. He says, “the determination of shared paradigms is not, however, the determination of shared rules. That demands a second step” (1970: 43). The search for rules “is more difficult than the search for paradigms” conceived as examples (Kuhn, 1970: 43). It is important to note here that each problem area has its own character and, to some extent, its own norms of what makes for a good explanation. (One might question the applicability of Kuhn’s sociological analysis to IR; for objections and responses see Chernoff, 2014: 12–15.)

Consensus on the dyadic hypothesis and its explanation

Over the last quarter century the most extensively debated set of questions in IR consists of those connected with the DP. The DP debate is one of the rare cases exhibiting scientific-style progress in the sense of approach-to-consensus on two principal hypotheses, the monadic and dyadic. The consensus is that the monadic hypothesis is false and the dyadic hypothesis, that pairs of democracies are unusually peaceful toward one another, is true; moreover, there is near consensus now that liberal theory provides an *explanation* for the latter. Scholars in IR learn the subject of a debate and the norms of argument in the Kuhnian way described in the previous section. This section offers support for the claim that there has been movement toward consensus on the best explanation, and the next section attempts to show how this coincides with greater overlap of the criteria for theory-choice among authors on all sides of the debate.

Given the description of how scholars are socialized into a particular debate over a research question, via the most influential or prestigious publications, the character of a debate at any given moment can be understood by examining the works that are most influential at that moment, whether they are older classics or newer publications. In identifying what the ten or so most influential works are, we need a set of criteria to pick them out from the large pool of publications. Three that come to mind are: number of citations; appearance on graduate IR course syllabi in the top doctoral programs; and results of a survey of journal editors of the top 20 IR journals. The latter two sources of data are based on results of the TRIP survey (Jordan *et al.*, 2009). An effort was made to identify the top ten works. A few were clearly in the top four or five, but there was no

obvious cut-off point at number ten. So in order to minimize skewing results, the three that were clustered together after number nine all are included.

Some decision must be made with regard to the geographical extent of the debate one is studying; if there is a focus on the English-speaking scholarly world, one might ask about students of IR who primarily read French and German sources, and their inclusion would likely alter the results to some degree. If Western Europe is included, one might ask about the debate in Russia. Had Russia been included, one might ask about Japan or China. So “the debate” has to be defined in a particular way. There is no one right answer here; the geographical or linguistic scope of the published-works evidence base may be chosen in any way whatsoever, so long as the author is consistent in the study and does not go beyond those parameters in drawing conclusions. The decision here is to focus on English language sources (although the editors surveyed are often not those whose first language is English), and the survey of top 20 doctoral program course syllabi is limited to the U.S. This does not produce a study that is “too narrow,” as long as the conclusions are not generalized to cover a wider field. Given the choice of scope just described, the DP publications that ranked as most influential, in chronological order, are: Small and Singer, 1976; Rummel, 1983; Doyle, 1983a; Mearsheimer, 1990; Bremer, 1992; Lake, 1992; Maoz and Russett, 1993; Owen, 1994; Layne, 1994; Spiro, 1994; Farber and Gowa, 1995; and Gartzke, 2007. Interestingly, and somewhat surprisingly, all are in the naturalist tradition broadly defined. Hence the possible variable “type of explanation,” as discussed in section 2, becomes a constant.

In the 1990s some realists denied the empirical claim that democratic pairs, that is, dyads, were more peaceful toward one another than other kinds of dyads. They came to acknowledge the low number of democratic dyads that have fought wars but still rejected liberal explanations; in their place they presented realist-based accounts involving balance-of-power calculations, structural factors like bipolarity (Farber and Gowa, 1995, 1997a, 1997b), the claim that democracies narrowly averted war by means of realist power calculations (Layne, 1994), and simply luck. But by the end of the decade the arguments on the liberal side were persuasive enough that the major journals rarely published articles denying the empirical claim of the dyadic peace or liberal explanations for it. The approach-to-consensus also involved a good deal of discussion of the meanings of the key terms and convergence on those meanings. (On the limits of choices of meanings, see Ish-Shalom in Chapter 8 of this volume.) Continuing opposition to the DP is identified primarily with critical IR as exemplified by Ish-Shalom and Sjoberg in Chapters 8 and 9, which address normative issues that transcend empirical findings.

Although liberalism has come to be widely accepted as the explanation for the behavior of democratic dyads, there remains some debate over which aspect of liberalism is most responsible, whether political regime structure or economic orientation. But this still represents a much greater degree of discipline-wide agreement than one finds on most other IR questions.

The dyadic hypothesis debate was a very important one, since it had both major policy implications – many new regimes were being formed at the end of

the Cold War when the debate was at its peak – and far-reaching theoretical implications, because its truth would be an immense blow to almost all forms of political realism. It drew in many of the most respected theorists. The near absence of published works in the top journals in the past decade or so on the dyadic hypothesis is not because of any decline of interest in the subject; IR journals are still replete with papers asking questions about democracy and peace, often presupposing the truth of the dyadic hypothesis, but very rarely questioning it. Over the past decade the five most pertinent journals (*International Organization*, *International Security*, *International Studies Quarterly*, *Journal of Conflict Resolution*, and *Security Studies*) have published thousands of articles, yet in contrast to the 1980s and 1990s, only a fraction of 1 percent raise questions about the truth or proper explanation of the dyadic hypothesis. Of the five, only one (*International Studies Quarterly*) has published any papers specifically on the dyadic hypothesis, and even then it has published just two. The paper by Gibler (2007) questions the dyadic hypothesis, and the paper by S. Choi (2011) assesses what the author regards as the two best and most influential explanations for the dyadic peace. The *American Political Science Review* published a theoretical critique of the arguments for the dyadic hypothesis just over a decade ago (Rosato, 2003), and in the next several years published several short replies by Doyle (2005), Kinsella (2005), Rosato (2005), and Slantchev *et al.* (2005). The *American Journal of Political Science*, not primarily an IR journal, published another paper by Gartzke (2007).

The two most persistent authors who have argued against the role of democratic regime-type are Mousseau and Gartzke. Mousseau (2002–2003, 2003, 2009, 2012) argues that in whatever way we operationalize the dependent variable, war/peace, a contract-intensive domestic economy is a more adequate explanation than democratic regime-type. Gartzke (2005, 2007) has argued that the empirical dyadic hypothesis is better explained by the capitalist nature of states. While the writings of Gartzke and Mousseau offer a strong dissent from the near-consensus of the majority of liberal democratic explanatory accounts, they work against realist accounts in that they see the internal structure of states as the key to the explanation of the dyadic behavioral hypothesis. Moreover, they still fall within the category of liberal explanations, because, as Gartzke puts it, “[c]lassical liberal theory provides two streams of explanation for peace, one focusing on the forms and practices of government, the other on free markets and private property” (2005: 29). Gartzke’s and Mousseau’s works focus on the economic side of liberalism in explaining the democratic dyadic peace.

Constant scrutiny is an important part of currently accepted theories in natural sciences no less than in the social sciences. At present, the DP debate has reached a consensus both that the dyadic hypothesis is true and that the best explanation is some form of liberalism. As noted, for a time realists rejected the empirical claim that democracies did not fight one another, but later accepted that and began to offer several realist/non-liberal explanations for the observable claim of the dyadic hypothesis. The current consensus is certainly not an end of the story; agreement may break down in future. Still, we can conclude that, at

this stage of inquiry, there is a scientific consensus on the dyadic hypothesis and on liberalism’s ability to explain it.

Consensus and criterial overlap

The question then arises as to how the DP debate has been able to exhibit approach-to-consensus. The central hypothesis of this chapter, H-Plur states that *when debates exhibit greater overlap of criteria of theory choice they are more likely to approach consensus explanatory answers – and when there is divergent reliance on criteria, they are unlikely to approach consensus*. The penultimate paragraph of section 2 listed the criteria most often found in philosophy of science debates on “explanation” and the previous section listed the dozen authors who are taken as influencing most strongly the discourse on the democratic peace debate. The authors used the criteria in the following way:

There are not enough data points for statistical analysis. But when the results of Table 5.1 are placed in a histogram, as in Table 5.2, a visual inspection reveals a great great deal of symmetry between the liberals’ and realists’ uses of criteria. This impression can be supported by some simple calculations using the basic logic of a chi-square test: the expected uses of each of the criteria, on the assumption of perfect criterial-use overlap, are compared to the actual uses of the criteria and by liberals realists.

The hypothesis H-Conv states that authors on opposite sides of a debate in IR are more likely to approach consensus if each side uses (roughly) the same criteria to evaluate theoretical explanations as the other side(s). Hence, in a consensus-approaching imaginary debate with five authors on either side, where each author uses ten criteria, we would expect to find that the 50 criterial uses on one side will exhibit roughly the same proportions of each criterion (simplicity, explanatory unification, range, etc.) as the 50 criterial uses of the other side; that is, if the total number of uses of, say, simplicity, is 8, then we would expect that

Table 5.1 Authors’ use of explanatory criteria and explanation of democratic behavior

Criteria of adequacy	S/S	Rum	Dyl	Msh	Br	Lak	M/R	Own	Layn	Spr	F/G	Gart
Empirical adequacy	X	X	X	X		X	X	X	X	X	X	X
Robustness		X			X	X	X	X	X	X	X	X
Falsifiability		X			X				X	X	X	X
Uncovers true causes			X	X	X		X			X		
Mechanisms			X	X				X	X			X
Predictive accuracy		X		X	X		X		X	X		
Greater range of its domain					X						X	
Explanatory unification						X						
Supports counterfactuals			X	X				X	X			
Simplicity					X				X			
Imparts understanding			X	X				X	X			X
Fits with background beliefs			X							X		
Theoretical orientation	R	L	L	R	L	L	L	L	R	R	R	L

Table 5.2 Realism, liberalism, and criteria

<i>Criteria of adequacy</i>	<i>Realism (5 explanations)</i>	<i>Liberalism (7 explanations)</i>
Empirical adequacy	S/S, Msh, Layn, Spr, F/G	Rum, Dyl, Gart, M/R, Lak, Own
Robustness	Layn, Spr, F/G	Rum, Br, Lak, M/R, Own, Gart
Falsifiability	Layn, Spr, F/G	Rum, Br, Gart
Mechanisms	Msh, Layn	Dyl, Own, Gart
Imparts understanding	Msh, Layn	Dyl, Own, Gart
Simplicity	Layn	Br
Supports counterfactuals	Msh, Layn	Dyl, Own
Explanatory unification	–	Lak
Greater range of its domain	F/G	Br
Uncovers true causes	Msh, Spr	Br, Dyl, M/R
Predictive accuracy	Msh, Layn, Spr	Rum, Br, M/R
Fits with background beliefs	Spr	Dyl
Total uses of criteria	25	33

either side would use it four times. The difference between the expected and observed uses is zero. In a debate in which there is little approach to consensus H-Conv tells us that we should expect to find very different proportions of specific criteria used by the two theoretical camps. If there is strong overlap in the criteria in a debate where there is no approach to consensus, the H-Conv would be falsified.

In non-imaginary debates there is no guarantee that the most influential works on opposing sides will be equal in number. In the DP debate, of the dozen most influential works, seven were on one side and five were on the other. Furthermore, there is no guarantee that the particular realists and liberals in a debate will have the same propensity to rely on criteria of theory evaluation; one group may invoke a lot more criteria than the other. So a test that follows the logic laid out in the previous paragraph would require that the analysis account for these two differences. Thus, the total number of criterial uses by one theoretical school should be compared with the total number of criterial uses of the other. If there are 100 total uses in a debate that moves toward consensus, and two-thirds of the overall total uses are by liberals, then we would expect that, for any *specific* criterion, the same proportion, two-thirds, of the uses would be by liberals.

With this in mind, we note the observed number of Realist uses of each criterion, in the DP debate and compare that with an expected value if both groups (1) used each criterion in similar proportions; (2) had the same propensity to cite specific criteria; and (3) were represented in equal proportions in the group of publications surveyed. (There is no need to repeat this for Liberalism, since there are only two groups and, since the sum of two will equal zero, the differences between the absolute values of the expected and observed will be identical.) This approximates the logic of a chi-square test, even though the assumptions of a mathematical chi-square test are violated by the data of this group. The results are presented in Figure 5.1.

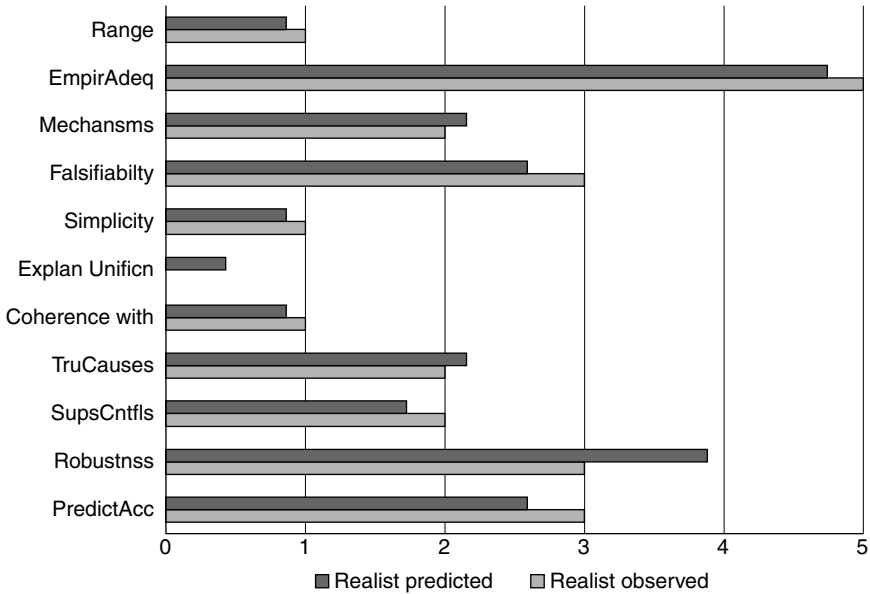


Figure 5.1 Democratic peace and criteria used by each explanatory theory

In the case of “uncovers true causes” criterion, the expected number of realist observations=2.15, and the observed number=2; the difference is 0.15. For “range,” the expected number of realist observations=0.86, and the observed number=1; the difference=0.14. When 0.15 and 0.14 are added to the other nine differentials the total=3.56. This sum can be divided by the total number of criterial uses by both groups (=58) to yield the average differential=0.061. (The smaller the differential, the greater the overlap of uses.) This quotient may be compared to the values for two other similar debates: nuclear proliferation and alliance formation. (These details of the calculations are available elsewhere in Chernoff, 2014: Chapter 6.) In the debate over balance-of-power theory, there is some movement toward consensus but much less than in the DP debate; there is no positive consensus but there is at least a negative consensus that pure balancing theories are inadequate. In that debate the criterial difference number=0.141, nearly twice that of the DP debate. And in the debate over causes of nuclear proliferation, where there is much less movement toward, since there is neither a negative nor positive consensus, the criterial difference is 0.222, roughly four times as great as the DP debate.⁷

The DP debate has exhibited more progress than most other IR debates. And authors in the DP debate share more reliance on particular criteria for a good explanation. So, thus far it appears that one of the reasons that there has been progress in the DP debate may well be the closer agreement on what makes for a good explanation. The similarity of criteria appears to have made possible the

convergence, as H-Conv asserts. The overlap of criteria enables authors on one side to address in direct ways the criticisms leveled by authors on the other side. When Layne, Spiro, Gartzke, and others published their criticisms of Russett and his coauthors, they were addressing clear and specific criteria of explanatory success, even if they did not always name them explicitly. The specificity of those criticisms led DP defenders to revise their research design and run new tests that made it difficult for critics to continue to publish arguments aimed at discrediting the liberal democratic explanation for dyadic peace – even if there are still disagreements about whether the political or economic features of democratic dyads plays a greater explanatory role. The critique from Harrison in Chapter 7 of this volume moves along a different dimension altogether, with an emphasis on parochialism of the DP in its currently overwhelmingly dyadic form.

One might object that the DP debate moved toward consensus because it was so heavily quantitative. And it seems intuitively plausible that works in quantitatively focused debates are likely to engage each other more directly than those in qualitatively conducted debates. This might be true, but there is no reason to conclude that this outcome is a result of the simple fact that numbers are involved. It is more likely a result of the fact that rational discourse in quantitative methods has led researchers in that approach to agree more or less on a set of criteria for superior explanations. Hence quantitative convergence is simply a special case of H-Conv. And there is no reason why other research approaches that overlap on criteria of evaluation cannot show the same epistemic progress.

Context and the questioner's intent

Progress is possible in non-quantitative debates if authors pay close attention to the criteria they use (and the criteria that their opponents use) and if they scrupulously clarify the precise question under discussion, which is the focus of this section. In some cases it is not clear what question an explanation aims to answer. That lack of clarity can stem from several sources, such as the intent of the questioner, the background knowledge and beliefs of the audience, and the area of greatest interest or concern to the audience. Various authors in IR and in the philosophy of science hold that an explanation does not exist in a vacuum but is developed in order to solve a particular puzzle or answer a particular question. The questions can be misunderstood if questioners and audiences do not pay sufficient attention to several key factors.

Suganami has developed one of the most insightful accounts of “causation” in IR and the causes of war. He makes several important points connected to the present inquiry about the possibility of progress in IR. First, Suganami argues there are different questions that one might have in mind when asking why wars occur. What would count as an acceptable explanation for war depends on which of the questions is intended. Suganami (1996: 33; 2002: 308) distinguishes the following questions: (1) What conditions must be present for there to be war at all? (2) What conditions are likely to give rise to war? And (3) how did this particular war come about? As he sees it, Waltz makes the case that “anarchy” is

the most fundamental cause of war by failing to separate different questions which, at different times, Waltz's theory appears to be designed to treat.

For Suganami, had Waltz consistently separated the first and the third questions, he "would have hesitated to place together in the first level of analysis such incongruous items as ... human nature ..., the personality traits of key decision-makers, and ... their specific thoughts and acts under specified circumstances" (1996: 34). Furthermore, had Waltz separated them,

he would not have treated a historical interpretation that a tense and unstable situation among particular states at a given time was the system-level cause, for example, of the First World War as though it were the same kind of assertion as his central thesis that the fact that there is nothing in the international system to prevent war was the permissive cause of war as such.

(1996: 34)

Suganami says that the precise formulation of questions, i.e., exactly what the questioner means by the question, will have a determinative effect on the sort of explanation that will provide an appropriate answer.

Suganami's argument has much in common with one developed some years earlier by Van Fraassen (1980). Van Fraassen explicitly rejects the idea that an explanation can be anything other than pragmatic. Consider a security studies scholar writing about the War of 1812, to use Achinstein's (1984) example. Such a scholar might raise a question about the observation of the American flag flying over Fort McHenry after the extensive British assault in 1812. For Van Fraassen, an explanation is an answer to a question; thus, one would need to pose a question to which an explanation of the flag's presence would constitute an answer, such as, "Why is our flag still there?"

However, on Van Fraassen's account, this question does not tell us all we need to know about what is being asked: knowledge of the context is essential. The questioner might be asking why our flag is still there (1) in contrast to *some other flag* being there (2) in contrast to our flag being *somewhere else*; or (3) in contrast to why our flag is there *instead of some other sort of object* being there. Which proposition forms the proper contrast to "Why is our flag still there?" determines just what that question is asking, which in turn determines what would count as an appropriate explanation of why our flag is still there. According to Van Fraassen (1980, 2002), the proposed explanation is only an appropriate explanation (and still may or may not be successful) when it is clear what the *contrast class* is, which the context will supply. Wendt also alludes to this element of explanation (1999: 88; cf. also Garfinkle, 1981; Woodward, 2003).

The questioner also may be asking for a sequence of events that led up to our flag still being there, or may be asking for the function or purpose of the flag being there. These two questions would then call for different types of explanations, i.e., historico-genetic and functional, respectively. Thus we also need to know the *relevance relation* of the question in order to know what would qualify as an explanation of "our flag still being there." Van Fraassen holds that there

must be several other elements beyond mere description for any set of statements to qualify as an explanation. He says:

The description of some account as an explanation of a given fact or event is incomplete. It can only be an explanation with respect to a certain relevance relation and a certain contrast-class. These are contextual factors, in that they are determined neither by the totality of accepted scientific theories, nor by the event or fact for which an explanation is requested.

(1980: 130)

In the DP debate there was a clear and delimited “contrast class” consisting of answers drawn from liberal theory and power-based realist alternatives.

Importance of the audience’s background knowledge and interests

What readers’ background knowledge and interests are affect what they will accept as an adequate explanatory answer. Suppose we compare a hypothetical academic conference in Chicago in 1998 to one in Beijing at the same time, where the scholars at either site consider the question “How do we explain that the very heated dispute between the UK and Iceland in the 1970s over fishing rights did not result in war?” The idea that both the UK and Iceland are mature liberal democracies would be well known by both audiences. But it would be regarded by the two audiences in different ways as an explanatory factor. The audience in Chicago would not believe there is much of a reason for the two democracies to go to war, given that it is common knowledge to the audience and to the publics in either country that the UK and Iceland are mature democracies. However, in Beijing, scholars may not accept as part of their background beliefs that democracies like the UK and Iceland, which are developed capitalist states, almost never go to war with one another. The Chinese scholars are well aware that capitalist states go to war with one another, in fact such states may be even more inclined to do so because of the very fact of their being capitalist. The background beliefs of the target audiences form the context out of which a question gains specific meaning.

This should not be construed as a way in which naturalism fails and to mean that the social sciences are less “scientific” than the natural sciences. Achinstein argues for context-relevance and pragmatics of explanation universally, in physics and in all scientific contexts. He presents an example of the context-dependence of answers to a question about atomic theory, namely the Rutherford experiment in 1911. Achinstein says, “Rutherford’s explanation is good ... because it answers a causal question about the scattering [of alpha particles] ... in a way that physicists at the time were interested in understanding the scattering” (1984: 286). The specific aspects of the process in which the community of investigators is most interested affects the appraisal of competing explanatory theories. Thus, when Rutherford presented his explanation, the audience (the scientific community of 1911) had precise interests in specific aspects of scattering.

And those interests were part of the context of the explanation and formed a part of the framework for what would constitute a superior explanation.

Context-dependence, progress, and the democratic peace debate

The precise meaning of the question, in ways beyond those that Suganami emphasizes, can make a difference in the meaning of the question and thus in the sort of explanation that would be regarded as sufficient by the community within which the question is posed. The fact that the democratic dyad debate was so intense over such a short period of time increases the chances that the aspect(s) of democracies' behaviors that the investigating community was most interested in would overlap to the greatest extent. In contrast, in the post-NPT nuclear proliferation debate, noted above, there was a steady stream of major works throughout the period.

The DP debate exhibits a pattern in which the most often-read works appeared within a very short time frame: ten of the 12 works were published in just 14 years. Only the earliest, Small and Singer, and the most recent paper included, Gartzke, are outside of the 1983–1996 period. It may be that when authors are writing in such close proximity to one another, the context in which they are writing is most likely to be unchanged and thus they are more likely to be interested in the same aspect(s) of the explanandum phenomenon, and thus more likely to interpret the explanandum phenomenon (alliance formation, acquisition of nuclear weapons, peace within democratic dyads) in the same way. At the time of the Rutherford experiment, the scientific community of particle physicists had a particular interest, which may have made it more likely that they would agree on the superiority of one explanation. So the closer temporal sequence of works in the DP debate may have enhanced a similarity of focus and framework, which may in turn have helped greater consensus develop as compared to the other debates.

The authors discussed above make a good case for question-ambiguity as an explanation for stalemate in some of the debates. Space considerations prevent further discussion of this idea here. But criterial disagreement is one factor perhaps among several that can inhibit movement toward consensus (see Chernoff, 2004, 2006). We may conclude, however, that any explanation in IR is an attempt to answer a question. But progress in debates over “the best explanation” requires that authors be extremely clear in how they formulate and explicate the central focus and meaning of any question they purport to be answering.

Conclusion – progress can occur

The explanation for the dyadic hypothesis shows that progress, in the senses of cumulation and approach-to-consensus, is possible. On the surface the fact that the debate was so heavily quantitative gives the appearance of an increased chance of progressive success arising from quantitative debates. However, this chapter has argued that there are philosophical and empirical reasons to believe

that two more fundamental features of the debate – criterial overlap and convergence on the precise meaning of the question – were more responsible for the outcome than the fact that the debate was quantitative. They are more fundamental because they are also the reasons why quantitative debates may have had an edge over qualitative debates in achieving agreement on best explanations. When questions are carefully formulated, including a statement of reference-class, and when criteria are explicitly identified, qualitative, interpretive, and constructivist work can produce cumulation and agreement. Thus the chapter has supported the hypothesis H-Conv *that when debates exhibit greater overlap of criteria of theory choice they are more likely to approach consensus explanatory answers – and when there is divergent reliance on criteria, they are unlikely to approach consensus.*

This leads to a research *recommendation* for IR authors to enhance the production and growth of knowledge, namely, that *authors should make explicit the criteria of theory evaluation that they use.* Since all authors rely on such criteria, usually implicitly, all that is needed is to make explicit what they are doing. This reduces the burden of opposing authors having to infer what those criteria are. When this is done, debate can be focused on the actual sources of scholarly conflict by explicating criteria. This clearer focus will allow disputants either to shift to rationally and philosophically grounded criterial debates, or will allow them to agree to disagree, but the point of dispute will no longer be a mystery. The chapter also has argued that the sorts of methods that are most appropriate will be subject to debate, but rational philosophical debate is possible once the subject matter is clearly stated: there is no need to retreat to separate the groups into “incommensurable” camps. Some questions, when properly explicated, are better approached by interpretive methods, some by naturalist methods, and still others by thoroughly moral-normative methods. Thus the chapter has argued for the hypothesis H-Plur *that inquiry in IR may proceed according to a range of legitimate methods, where the nature of the question at issue affects the choice of the most appropriate methods and where all methodological principles are truly public in that they are open to meta-theoretical criticism from all reason-based arguments.* The bounded pluralism described here leaves open the prospect for rational debate between methodological and substantive-theoretical schools, and at the same time raises awareness of the fact that in different arenas of inquiry different methodological approaches will win the rational debate over the best method.

Certainly many authors are engaged in debates in which neither side is clearly wrong; in some cases the disputing authors are focused on different aspects of a research problem, including cases where some authors are asking about more empirical aspects of the case while others are asking about more moral-evaluative aspects. What this chapter has tried to show is that once authors are careful in clarifying questions under discussion and are explicit in clarifying the exact problem and in stating the criteria on which they rely to defend their conclusions, which we have seen them do in the DP debate, the door is open for much greater epistemic progress and the growth of knowledge in IR. And all of

that is consistent in its implications with sociable pluralism as enumerated by Freyberg-Inan in Chapter 4 as a unifying theme for the volume.

Notes

- 1 It cannot be emphasized enough that there are many disagreements in IR that do not allow us to infer that one side is simply wrong; very often two or more disputants are dealing with different aspects of a question. The social world is complex with many orthogonal forces that do not allow superficial arguments as to who is right and who is wrong. And “Cross-cutting theories may be used to solve related problems” (Chernoff, 2005: 216). There are cases where the two or more parties have the same evidence and understand and define the (purely empirical or minimally-moral) question in the same way (2005: 210); and they disagree in such cases, if one party is right, others are wrong. The reduction argument is left for the reader to consider, namely, the consequence of treating S’ as just as correct as S.
- 2 Walt, 2013 is a rare exception.
- 3 All of the above is within the bounds of the philosophically dominant fallibilist understanding of knowledge. In recent decades even foundationalists like Audi (e.g., 1993) endorse fallibilism; that is, even those who defend a foundation of certainty for knowledge (to avoid skepticism or regresses) allow that many propositions built on the foundation are knowable but are not immune to future revision and scrutiny and possible falsification.
- 4 For a comparison of the three debates see Chernoff (2014).
- 5 The position I support (2005: e.g., 18–19, 30, 41–48) also is conformity with the core argument of Ish-Shalom’s in Chapter 8 of this volume, according to which the purely empirical and the purely evaluative are not always possible to separate in practice.
- 6 Jackson permits meta-theoretical debate within each of the four cells but not across his two dividing lines (this volume, Chapter 1). But philosophers who investigate questions of being and knowledge draw fundamental distinctions in ways different from Jackson. It is not clear that those two distinctions should be taken as the ones that define the cross-cell firewalls rather than other authors’. On another issue, Jackson is on strong ground in holding that what is “scientific” is relative to the context of the epoch in which the claim is made. But at any point in time it must bear the three characteristics, including “publicity.”
- 7 When we consider the most influential works of the past decade in the debate over why states build nuclear weapons, those by Hymans (2006), Solingen (2007) and the quantitative works of Singh and Way (2004) or Jo and Gartzke (2007), we see that they exhibit very little convergence. There is not only an absence of agreement on the best explanation in terms of specific causal factors, there is also no agreement even on the level of analysis that can guide us in finding the best explanation. Hymans uses first-image individual psychology of leaders, while Solingen combines second-image regime-orientation with third-image system factors such as norms of the regional international system (e.g., the Middle East has a different set of norms from the East Asian system). While there is no space to consider the issue, the analysis of the top ten authors in the proliferation debate that I have done elsewhere (2014) concludes that there is far less overlap in the uses of criteria.

6 Systemism, analytic eclecticism, and the democratic peace

Jarrold Hayes and Patrick James

Overview

This chapter assesses progress in the democratic peace research enterprise. While not a new endeavor – many scholars have produced insightful reviews of the democratic peace – the *modus operandi* here differs.¹ Specifically, we combine analytic eclecticism and systemism, two approaches toward research that previously had not been connected, to obtain a fuller sense of causation in the democratic peace research program. Thus, in addition to exploring the democratic peace agenda in a novel way, this chapter operates within the context of philosophy of inquiry to demonstrate the value arising from a union between systemism and analytic eclecticism.

Over the last three decades, the democratic peace (DP) stands as the most sustained program of research in International Relations (IR). Research has “exploded” (Hayes, 2012: 2) and “cumulative knowledge is considerable” (Liebel, 2011: 384). “The democratic peace,” as described by Liebel (2011: 386) among many others (e.g., Chan, 2010: 5928), “is a dyadic theory, as opposed to a monadic theory, because it is concerned with a pair (dyad) of democratic states and how they act in accordance with one another.” Evidence for the dyadic version of the democratic peace is substantial (Ungerer, 2012: 16). In Chapter 5 of this volume, Chernoff establishes the near-universal acceptance of the dyadic proposition as the product of a debate that featured agreement on criteria for evaluation.

Questions for the democratic peace have moved beyond establishing an association between regime-type and conflict to revolve around questions of theory and causal mechanisms as well as the scale of the democratic peace effect. These tendencies are a welcome development and, as will be argued emphatically by Harrison in Chapter 7, offer the optimal path to progress to the extent that levels of aggregation beyond the dyadic are brought into consideration.²

While presented in detail later in this chapter, we briefly introduce analytic eclecticism and systemism here. Previously, these frameworks have been implemented separately. Both hold promise for further application, the argument being that a synthesis of ideas from analytic eclecticism and systemism enhances the significance of both approaches.

Analytic eclecticism is “an alternative way of thinking about the relationships among assumptions, concepts, theories, the organization of research, and real-world problems” (Sil and Katzenstein, 2010: 2). An eclectic approach is defined as one that “*seeks to extricate, translate, and selectively integrate analytic elements – concepts, logics, mechanisms, and interpretations – of theories or narratives that have developed within separate paradigms but that address related aspects of substantive problems that have both scholarly and practical significance*” (2010: 10).³ Following analytic eclecticism’s pragmatic views on how and why to study world politics, Sil and Katzenstein are interested in engaging theory with practitioners. To move beyond paradigms, the approach focuses on solving problems “as they are understood by political actors” and without adherence to “scholarly conventions or theoretical boundaries” that guide and constrict paradigm-based research (2010: 10). At this stage of development, analytic eclecticism provides principles for conducting research but does not include a method for application.⁴

Systemism’s essence is conveyed by its most long-standing exponent, Mario Bunge (1996, 1998): a commitment to building comprehensive theories. Systemism transcends individualism and holism as the other available “coherent views” with respect to operation of a social system (Bunge, 1996: 241; see also H. Choi, 2011: 29–30). Rather than theorizing at the level of the system (holism) or its components (reductionism), systemism allows for linkages operating at macro- and micro-levels, along with back and forth between them, as well as facilitating comparison of alternative visions regarding cause and effect. Thus systemism is an *approach* rather than a substantive theory (Bunge, 1996: 265). As assessed in a recent and favorable review from H. Choi (2011: 29), systemism “has emerged as an important worldview and methodological approach in social science.” Systemism as a method emphasizes diagrammatic exposition of causal mechanisms. While the idea of “more box and arrow diagrams” might sound banal, in the context of systemism it is not. Systemism imposes rules for visual conveyance of causal mechanisms that promote clarity and rigor.

These summaries suggest gaps in both approaches that might be addressed through a synergy between systemism and analytic eclecticism. The apparatus of systemism – a commitment to fully specified theoretical linkages within and across macro- and micro-levels of analysis, along with diagrammatic exposition – can be of value in charting the direction of scholarship but lacks a specific framework for integration across multiple theories. Conversely, analytic eclecticism provides an integrative perspective on scholarship in IR – an agenda for inquiry – but lacks a method for implementation. Each approach then contains strengths that compensate for weaknesses in the other. We explore the benefits of this partnership further in subsequent sections, all in the context of using the combined strengths of systemism and analytic eclecticism to assess progress for the democratic peace.

This chapter unfolds in four additional sections. The second section presents analytic eclecticism as a perspective within which to evaluate the democratic peace. Section three focuses on how systemism can be used to convey causal

mechanisms within the democratic peace and thereby facilitate assessment of research progress through a synthesis with analytic eclecticism. The fourth section uses systemism to assemble the causal mechanisms identified by reviews of the democratic peace and then assesses the resulting expositions on the basis of criteria drawn from analytic eclecticism. Finally, the chapter evaluates progress in the democratic peace research enterprise and identifies directions for future study.

Analytic eclecticism

Three markers of eclectic scholarship are identified by Sil and Katzenstein (2010: 19). We introduce each in tandem with an illustration from democratic peace research.⁵ The examples suggest that the democratic peace program of research has evolved consistently with the principles of analytic eclecticism. Discussion of analytic eclecticism then turns to priorities, value-added and what the approach does not entail.

Analytic eclecticism's first marker is an open-ended approach toward problem formulation. This process should encompass the "complexity of phenomena" and is "not intended to advance or fill gaps in paradigm-bound scholarship" (2010: 19). Thus, research programs should draw on ideas from a range of theories in a focused effort to confront the complexity of real-world phenomena. Complexity is not held up for its own sake, but rather as a reaction to the facts that human societies are indeed complex and only by embracing that complexity can scholars link theory with reality. For example, within the DP, scholars have turned to perceptions of democratic identity and causal mechanisms related to civil–military relations in an effort to make sense of the quantitative finding that democracies do not use force against each other (Hayes, 2012: 17). These factors originate within different disciplinary matrices – psychology and political science, respectively – but both are found to matter in different ways with respect to the presence of pacific interstate dyads. Thus problem formulation and analysis in the democratic peace transcend the boundaries of a given discipline or theories within it; solutions are to be gathered from wherever insight may be gleaned.

Second among the markers for analytic eclecticism is development of a "middle-range causal account" that incorporates "complex interactions among multiple mechanisms and logics drawn from more than one paradigm" (Sil and Katzenstein, 2010: 19). In that context, consider the list of "promulgated variables" that Ungerer (2012: 12) identifies but then *excludes* from the democratic peace research program because of a desire to focus on the role of democracy.⁶ From the standpoint of analytic eclecticism, however, incorporation of factors taken from diverse paradigmatic backgrounds is not only acceptable but worth pursuing. Middle-range theorizing for a given set of dependent variables may work best with a diverse set of explanatory factors that transgress paradigmatic boundaries. The burden of proof, moreover, falls on critics of analytic eclecticism, who need to show why efforts to combine propositions from different theories and even paradigms should *not* be pursued.

Third among the markers of analytic eclecticism is an emphasis on practical relevance; a goal is to obtain findings that “pragmatically engage both academic debates and the practical dilemmas of policymakers” (Sil and Katzenstein, 2010: 19). This marker works well for the democratic peace. As the research program has progressed, it has incorporated many matters with obvious relevance to policy, such as sanctions, cooperation and settlement through negotiation, and war fighting (Ungerer, 2012: 13). These are incorporated as concepts to capture actual policy practices, but also contain at least implicit elements aimed at informing policymaking.

What, then, are the priorities of analytic eclecticism? The approach “trains its sights on the connections and interactions among a wide range of causal forces normally analyzed in isolation” (Sil and Katzenstein, 2010: 12). This priority is manifested in democratic peace research as summarized by Hayes (2012: 17). Hayes identifies two “waves” of theorizing that bring together disparate explanations of the democratic peace. The first emphasizes domestic political structures (e.g., separation of powers, credible signaling), which encourage pacific interstate dyads by slowing down the pace of policymaking and creating a greater opportunity for diplomacy to work. These explanations are grounded in rational actor and game theory. The second wave focuses on sociological elements, notably democratic norms and identity, often emerging out of constructivism. The key point is that both waves emerge out of different research traditions in IR that often operate in isolation from (and conflict with) each other.

While integration of these two waves within the DP literature is far from perfect, their joint place within the quest for explanation of the democratic peace typifies analytic eclecticism. The goal here is not eclecticism for its own sake, but rather eclecticism as a means through which scholars access a wide range of tools for understanding outcomes. In keeping with Hayes’s waves, neither alone provides a sound basis for understanding the DP. To achieve that goal, concepts and theories present in both waves are necessary.

Analytic eclecticism attends carefully to processes that “cut across different levels of analysis and transcend the divide presumed to exist between observable material factors and unobservable cognitive or ideational ones” (Sil and Katzenstein, 2010: 21).⁷ Thus analytic eclecticism would seem to line up with scientific realism as advocated by Wight in Chapter 2. (Un)observables can coexist within its boundaries.

Diversity in levels of analysis and causal factors is clear to see in democratic peace research. Hayes (2012), for instance, combines material and ideational factors proposed to explain the democratic peace. Although the DP literature is far from ideal in terms of bridging the material-ideational divide, the presence of both within the broader research agenda fits with an analytic eclecticist mandate. Hayes also points to the presence of analysis on multiple levels and progressive advancement in that regard as research expanded beyond an overwhelmingly dyadic mode of analysis to include system-based explanations that focus on the a system-level influence of democratic norms (Hayes, 2012: 12, 17; see also Harrison, Chapter 7, this volume).

Sil and Katzenstein (2010: 22) advocate the development of middle-range theories that “shed light on specific sets of empirical phenomena” and “do not aspire to offer a general model or universal theory that can be readily adapted to investigate other kinds of phenomena.” The democratic peace research program, while not aimed explicitly in that direction, ends up in line with the priorities of analytic eclecticism. Statistical models seek to explain a range of aspects related to why democratic dyads have properties different from those that are autocratic or mixed. The modal study in the democratic peace program tests a multivariate model with quantitative data from the last two centuries on conflict processes. Thus the democratic peace research program takes an empirical generalization about interstate dyads and proceeds incrementally through inclusion of an expanding number and range of explanatory variables. As opposed to moving forward under a grand theory, the emphasis of democratic peace research is on testing propositions.

Analytic eclecticism “encourages the construction of theories or narratives that generate ‘pragmatic engagement’ with the social conditions within which prevailing ideas about world politics have emerged” (Sil and Katzenstein, 2010: 22). Thus, analytic eclecticism is rooted in the “real world,” and derives its agenda from a desire to better link the abstractions of theory to complex outcomes that populate the international system. Democratic peace research reflects that priority. Both elite and mass opinions express ongoing interest in the role of democracy in shaping the world. This curiosity became greater with the collapse of the Soviet Union, which had been “team leader” for the autocratic vision of the future. Today, news stories that focus on the importance of democratization are legion. The utter absence of democracy in North Korea, coupled with its abysmal failure, requires no academic training to comprehend.

What is the value-added of analytic eclecticism? Sil and Katzenstein (2010: 20) emphasize “expanding the scope and complexity of questions so as to facilitate a more open-ended analysis that can incorporate the insights of different paradigm-bound theories and relate them to the concerns of policymakers and ordinary actors”; the approach brings “attention to the multiplicity, heterogeneity, and interaction of causal mechanisms and processes that generate phenomena of interest to scholars and practitioners.” It provides an agenda for developing analytical frameworks by directing scholars to engage theory across paradigmatic boundaries with how real-world actors understand the world. Thus, analytic eclecticism provides a guide for IR scholars thinking about the relationship between theories and concepts and a foundation for developing causal mechanisms that emphasize grounding in the real world of politics.

Democratic peace research exemplifies the preceding components of added value from analytic eclecticism. Questions arising within the research program expanded from exploring a basic correlation involving democracy and peace at the dyadic level to include factors from a widening range of paradigmatic origins: liberal, constructivist, and even English School with the recent system-oriented turn toward a focus on norms transmitted through international society. Democratic peace gradually added policy-relevant items, such as identifying

conditions related to intervention and conflict escalation, to its agenda. Furthermore, as Ungerer (2012) points out, causal mechanisms investigated have expanded to include a reversal of the arrow; i.e., what are the implications of dyadic peace for democracy?

Sil and Katzenstein (2010: 23) sum up analytic eclecticism as follows:

Our conceptualization of eclectic scholarship is distinctive in that it seeks to bridge *all* of these concerns, linking a pragmatist orientation towards the production of useful knowledge to problem-driven research aimed at better understanding of real-world phenomena and to mid-range causal accounts that draw upon mechanisms and processes normally analyzed in isolation within separate paradigms.

A natural question to ask next is this one: “Given its encompassing nature, what does analytic eclecticism *not* include?” “Analytic eclecticism,” Sil and Katzenstein (2010: 16) are careful to note, “does not imply that ‘anything goes’.” A need for logical consistency among diverse mechanisms is implicit. Otherwise, internal contradictions would take analytic eclecticism down the path to scientific irrelevance identified by Vasquez (1999). This point will re-emerge when systemism is put forward as a potentially valuable partner for analytic eclecticism in assessing progress for the democratic peace.

Analytic eclecticism is neither “theoretical synthesis” nor “coterminous with multi-method research or methodological triangulation” (Sil and Katzenstein, 2010: 17–18). The first assertion makes immediate sense because of the effective impossibility of synthesizing diverse paradigms into one; the result would be a Frankenstein’s monster of self-contradiction. The second claim opens the door to the question of how method *does* come into play. How is analytic eclecticism to be implemented? Its very nature argues against orthodoxy, so it is not obvious how to proceed.

Sil and Katzenstein (2010) show analytic eclecticism in action via examples of research throughout IR: on security studies, political economy, and order and governance. These illustrations, which are compelling, suggest that successful research consistent with analytic eclecticism already is plentiful. So, too, do the examples provided earlier from democratic peace research. Yet the discussion in Sil and Katzenstein (2010) concludes with a sense that an “invisible hand” is at work. Research projects do not *intend* to pursue an eclectic approach, but if they apply it creatively, good things can happen.

Table 6.1 conveys a few selected statements (numbered I to IV) from Sil and Katzenstein (2010) that pertain to implementation of analytic eclecticism. Statement I asserts that intellectual context should be taken into account. Elements from major paradigms – currently liberalism, realism, and constructivism – should be used to build complex middle-range theories that maintain relevance to the real world. From Statement II comes the admonition that it is essential to know how variables from across paradigms are connected. Statement III calls for judicious attention to how inter-paradigmatic causal mechanisms interact and

Table 6.1 Analytic eclecticism and implementation

<i>Number</i>	<i>Statement</i>
I	The specific contours of this strategy depend on the relevant intellectual context... In the context of contemporary international relations, analytic eclecticism is minimally operationalized as analysis that extricates and combines elements of theories embedded in the three major paradigms – realism, liberalism, and constructivism – in the process of building complex middle-range causal stories that bear on important matters of policy and practice.
II	Quotation from T.V. Paul: “We need to know how these different variables (often drawn from different paradigms) are connected, and how they affect or cause the outcome – alone or in conjunction with others – that we are trying to explain.”
III	What makes an eclectic approach rigorous is not the incorporation of each and every imaginable factor, but judicious attention to how a set of clearly defined causal mechanisms normally posited in different paradigms interact with each other and combine to generate interesting outcomes.
IV	It is not possible to construct a definitive “model” or “guide” for conducting eclectic scholarship.

Source: Sil and Katzenstein (2010: 37, 89, 100, 205)

combine to form a more convincing whole. Statement IV warns against attempting to develop a model or guide for eclecticism – a point that would seem in line, perhaps, with the very nature of the approach itself.

Analytic eclecticism, however, will be in a position to contribute even more to IR if it can be linked to a method that preserves its appealing traits while creating the potential for more effective implementation. It will be argued that systemism is precisely the method that is called for here.

Systemism

For systemism, with its emphasis on causal mechanisms, the point of departure is that correlation does not equate with causation. Consider the adage about storks and birth rates as a simple illustration of what is at issue here. The correlation is present, but not because storks, as parents might tell children, are bringing babies. Instead, a causal mechanism involving rural areas is at work. The degree to which an area is rural will explain both the frequency of storks being observed and the birth rate. In essence, systemism does not focus on obtaining ever-larger coefficients from regression-style statistical research or accumulation of case studies that show association. Instead, systemism goes beyond debates over methods to focus explicitly on causal mechanisms.

Consider the assessment of social science from Pickel (2007: 392): “abandoning conceptions of systems has imposed a high price on the social sciences: a lack of ontologies and methodologies that are both philosophically profound and

scientifically defensible.”⁸ The concern becomes successfully achieving *mechanismic explanation*. Without a system orientation, a mechanistic account cannot be achieved; instead, research becomes an exercise in increasingly sophisticated empirical study that cannot depict cause and effect directly. This is because non-systemic research focuses only on an element of the overall system of interaction and causation that produces the end result. In so doing, it misses the mechanisms that are necessary to establish a causal account.

Returning to the stork example, failing to take account of the broader system and the mechanisms operating within the system would result in scholarship that ever more carefully charts the correlation between the presence of storks and birth rates, but with no engagement with causation. Thus, a quest for causal mechanisms entails system-oriented thinking. The ontology and method of systems analysis open up new vistas to research. A system orientation serves as a reminder that causal effects often may be nonlinear; regarding method, such thinking issues a prompt that causal relationships “cannot be inferred from linear correlations” (Pickel, 2007: 394). Furthermore, systems analysis offers a way beyond the tedious debate over material versus ideational analysis. As Pickel (2007: 404) asserts, “social systems cannot be directly observed” and therefore a thorough exposition must include composition, structure, environment, and causal mechanisms. For such reasons, any ensuing argument about whether idea-based or material explanations are paramount is irrelevant.⁹

Systemism, moreover, is compatible with analytic eclecticism’s emphasis on building mid-level theory (Kesgin, 2011: 337). The creator of systemism, Bunge (1996: 264; see also 1998), puts it forward as a way to think about the world; in principle, it can be applied to any aspect of social life:

The alternative to both individualism and holism is systemism, since it accounts for both individual and system, and for individual agency and social structure. Indeed, systemism postulates that everything is a system or a component of one. And it models every system as a triple (composition, environment, structure), or CES for short, so it encompasses the valid features of its rivals [individualism and holism].

To the CES apparatus Pickel (2007) adds causal mechanisms to convey explicitly the contribution made by the diagrams entailed by systemism, resulting in what this study will refer to as CESC.

Composition refers to participants – who may be individuals, social groups, or those speaking for government institutions – and the interactions among them. The *environment* is everything beyond the boundaries of the system. It may have an influence on the system and vice versa. *Structure* refers to the rules that govern interactions among participants. Finally, a *causal mechanism* is a link through communication or material action involving variables within the system.¹⁰

From the standpoint of systemism (Bunge, 1998: 65), the choice between unit and system is a flawed dichotomy and that point is well recognized already in IR

(Brecher, 1999; James, 2002a, 2002b). All theories must be expected to deal with both systems and units – that is, the macro- and micro-levels – in some way. This is apparent in the discussion of ideas about the democratic peace that opened this chapter. Macro- and micro-levels influence each other; they do not operate in isolation (Hayes, 2009; Archer, 1996: xxi).¹¹ The question is how best to put together a theory that takes this reality into account.

Systemism conceives of systems as entities nested within one another as sub-systems, systems, and supersystems (Bunge, 1996: 270). Thus work begins with identification of boundaries. Within IR, for example, the usual focus is upon levels of analysis, with international, state, and individual as near universal in usage after the highly influential treatment from Waltz (1959). However, as will be demonstrated through later application of systemism, the Waltzian language can produce confusion when efforts are made toward the all-important objective of integration vis-à-vis explanations. Put differently, the terminology developed below provides more rigorous boundaries for classifying variables as being in one category or another and thereby creates a better platform for articulating comprehensive explanations.

Systemism goes beyond holism and reductionism through inclusion of all basic connections that make up a theory.¹² The approach entails a commitment to “understanding a system in terms of a comprehensive set of functional relationships” (James, 2002a: 131). Figure 6.1 depicts functional relations in a social system from a systemist point of view. Variables operate at macro- (X, Y) and micro- (x, y) levels. Outside the system is the environment (E). The environment can be expected to provide inputs into, and experience outputs from, the system.

Within a system as depicted by Figure 6.1, four basic types of linkages are possible: macro–macro ($X \rightarrow Y$), macro–micro ($X \rightarrow x$), micro–macro ($y \rightarrow Y$), and micro–micro ($x \rightarrow y$). In addition, the effects may go back and forth with the environment, such as $E \rightarrow X$ or $y \rightarrow E$. In this figure, and in the subsequent

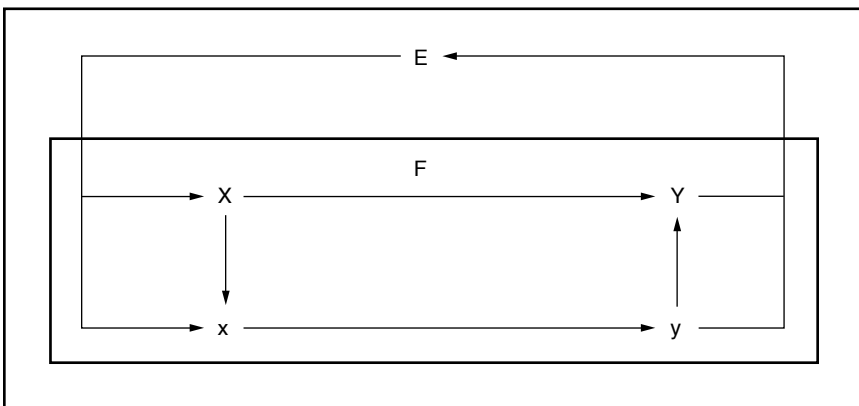


Figure 6.1 Functional relations in a social system

Sources: adapted from Bunge, 1996: 149; see also James, 2012

diagrams that focus on the democratic peace, upper- and lower-case characters correspond to macro- and micro-level variables, respectively. It is difficult to specify in advance how causation travels between systems. The nature of causation depends on the specification of causal mechanisms, which are identified by scholars addressing specific systems.

Note at this point the consilience involving the exegesis of scientific realism from Wight (Chapter 2, this volume) and systemism as a means of communication. As per the position advocated by Wight, systemism emphasizes specification of causal mechanisms. It is consistent with an intellectual position that is more demanding than instrumentalism or conventionalism. Systemism, in a word, is realistic; its diagrams are in line with Wight's emphasis on theories as maps, along with his favorable views on the value of visual representation of argument in and of itself.

Systemism entails and encourages specificity with regard to causal mechanisms. Of particular interest is functional form; what, for example, is the nature of $Y=F(X)$? Is the function incremental or something else? This question must be answered for all connections identified in the system. Assessment of Y as a function of X , by intuition, begins with an incremental or linear relationship, with complexity added as necessary. For example, some linkages may be incremental, such as water cooling down or heating up, and then step-level, with temperatures of 0 and 100 degrees Celsius resulting in freezing and boiling, respectively. Functional form also is important in strengthening the falsifiability of a theory by increasing the specificity of its causal mechanisms.¹³

Figure 6.1 is generic and may apply to any social system. Its contents will be elaborated through illustrations from the democratic peace. The linkages conveyed by Figure 6.1 – (1) between the environment and the system and (2) within the system itself – will be covered in turn. This process will reveal how the contents of any theoretical exposition can be translated into the language of systemism, which facilitates comparison.

What about systemism's emphasis on composition, environment, structure, and causal mechanisms (CESCM) at a general level and in relation to the democratic peace? Consider an example of a social system from IR. Analysis begins by identifying a system relative to a supersystem and subsystems, in other words, the system's boundaries. Hayes (2009) identifies a democratic peace system that positions the phenomenon at the macro-level. At the micro-level, Hayes identifies a specific domestic political relationship and mechanism – the political relationship is between securitizing actors and the target audience, and the mechanism emphasizes the role of shared democratic identity in constraining securitizing moves. Thus, the inter-democratic peace operates at the macro-level, which means that the micro-level emphasizes dynamics within states that generate the macro-level phenomenon. This system clearly exists within the context of subsystems (e.g., education systems, media) as well as supersystems (e.g., anarchy, alliances, regions). The key to systemism is identifying or specifying the system, how it functions, and how it interacts with sub- and supersystems. Hence, when applying systemism, this part of the process is empirical and

problem-driven. Causal mechanisms, such as communication between states, are conveyed through the preceding sample of connections. This completes the use of the democratic peace research program to illustrate composition, environment, structure, and mechanistic causation for a social system. Systemism has been introduced in full as a complement to analytic eclecticism.

With its emphasis on causal mechanisms and diagrammatic exposition, systemism is compatible with theoretical articulation within and across paradigms. The approach puts a premium on logical consistency. Systemism requires clarity about levels of analysis in tandem with cause and effect, so it fills a key gap in analytic eclecticism. *Systemism provides a method for implementation of analytic eclecticism that can convey any inter-paradigmatic exposition in a way that facilitates progress.* However, systemism also benefits from analytic eclecticism's stress on cross-paradigmatic theory building and emphasis on linking theory with how the world is experienced by actors in it. These elements help ground the mechanisms of systemism and prevent systemist analysis from becoming trapped within a specific paradigm. Thus the combination of analytic eclecticism and systemism is in line with sociable pluralism – the unifying theme for this volume as articulated in Chapter 4 from Freyberg-Inan – as a way toward progress in IR.

Assessment of the democratic peace

For purposes of evaluation, a series of review essays is consulted, with an explicit focus on expositions that at least partially discuss causal mechanisms. We identify four important reviews as the basis for analysis: Chan (1997), Chan (2010), Hayes (2012), and Ungerer (2012). The first review took place at a time when democratic peace studies had reached a point of critical mass, whereas three more recent efforts, in close proximity to each other and from a range of viewpoints, attest to the sustained importance of this research enterprise on regimes and conflict.

Diagrammatic reconstruction of the democratic peace from the narrative in Chan (1997) appears in Figure 6.2. This figure, like the others that follow, uses the region to define boundaries for the system; this will facilitate visual presentation while making no difference from the standpoint of conveying causal mechanisms. Thus the environment corresponds to the world beyond regional boundaries. Interstate relations constitute the macro-level, with the micro-level corresponding to interactions within states.

Pacific interstate dyads are the focal point for the democratic peace and appear at the center left. Micro–micro, micro–macro, and macro–macro connections, along with a linkage to the environment, are covered in turn. One macro–micro connection appears at the end as a point of speculation for the research program as it existed in 1997.

Two micro–micro links appear on the lower left. Climate impacts upon the likelihood of a democratic regime. Arid and insular territory is more favorable to autocracy than democracy. When democracy does arise, this regime type, in turn, shapes the role of society. Democracy facilitates a role for public opinion.

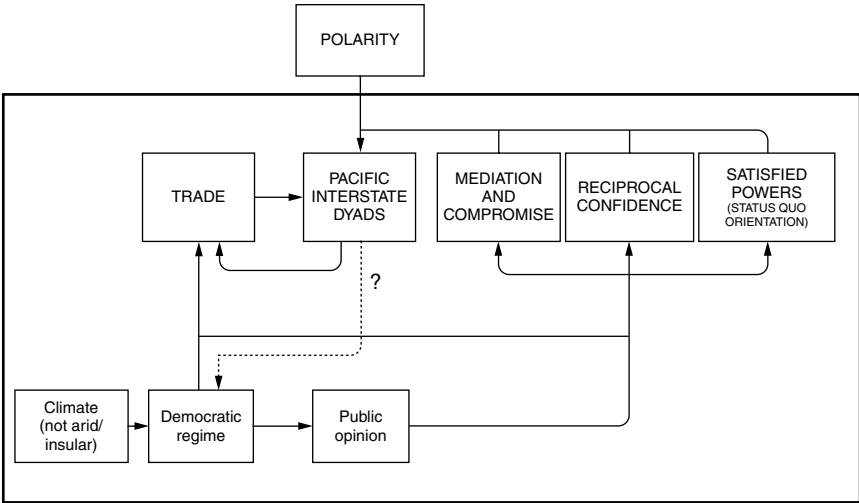


Figure 6.2 The democratic peace in 1997

Source: adapted from Chan, 1997

Micro–macro connections then ensue. Public opinion and democratic regime both feed positively into TRADE. Both state and society in a democracy will seek the gains that can be made from such exchange. On the right side of the figure, public opinion impacts upon SATISFIED POWERS (status quo orientation), RECIPROCAL CONFIDENCE and MEDICATION AND COMPROMISE. The first of these linkages reflects an expected disposition on the part of the public against an aggressive, warlike foreign policy. The second and third are projections outward of the free exchange of ideas expressed through public opinion. Governments should be able to bargain more effectively, in good faith, as a result of public input. Confidence should build within a dyad, accompanied by bargaining outcomes that result from a culture of openness to mediation and willingness to compromise.

Further connections take place at the macro-level, as SATISFIED POWERS (status quo orientation), RECIPROCAL CONFIDENCE and MEDICATION AND COMPROMISE promote PACIFIC INTERSTATE DYADS. A further macro–macro linkage is from TRADE to PACIFIC INTERSTATE DYADS. This connection goes all the way back to the philosophical exposition of Kant. The one feedback loop in the figure is from PACIFIC INTERSTATE DYADS into TRADE.

Finally, the environment impacts upon the likelihood of PACIFIC DYADS in the form of POLARITY. Cold War bipolarity is the mechanism here. Bipolarity enhanced the democratic peace because it encouraged solidarity between and among such states in the face of the communist threat.

Chan (1997) questions whether PACIFIC INTERSTATE DYADS feed back into the prospects for democracy itself. The figure represents this with an arrow made up of a broken line with an attendant interrogation point.

Figure 6.3 reveals the progress made in the research enterprise by 2010–2012. Striking is the intricacy of the figure in comparison to 1997. *Its complexity borders on unmanageable, for purposes of retention, in a narrative form.* Thus the sheer number of linkages serves as an argument in favor of using a diagram as the form of expression for causal mechanisms. Analysis will begin at the micro-level and move upward from there.

PACIFIC INTERSTATE DYADS anchor the figure at the center left. At the center right of the diagram, a set of micro–micro relationships is presented most easily by starting with democratic regime. Interlocking micro–micro linkages involving democratic regime appear on the lower right. Democracy and civil–military relations impact upon each other. Democratic governance encourages civilian control of the military and vice versa. Media openness also is synergistic with democracy. Open communication enhances debate and can shed light on efforts to dismantle democratic institutions. At the same time, democratic government encourages the free expression of ideas.

Across the page, further micro–micro connections ensue from democracy to five sets of characteristics. Consider each linkage, from right to left. Democracy affects the size of the selectorate. The effective governing coalition will be larger in a democracy. Democracy also will be conducive to separation of power and establishment of opposition parties. For the individual citizen, democracy will promote personal freedom and a society based upon liberal rationality, as opposed to a collectivist ideology enforced through autocracy. Further effects from democracy include political accountability, audience costs, and public constraints. These interlocking characteristics reveal efficacy for society in relation to the state. The public can hold leaders accountable and exert influence at the ballot box. The final trait in society is de-securitization. This refers in a democracy to the positive beliefs and dispositions that exist among citizens toward other states perceived to be democratic. Foreign policy toward such states is regarded as de-securitized.

Micro–macro linkages occur at the right and left sides of the figure. Return now to the mutually reinforcing complex of democratic regime, civil–military relations and media openness on the figure’s right side. These factors combine to influence a set of five macro-level entities. Starting at the far right, OBSERVANCE OF DEMOCRATIC NORMS (etc.), TRANSPARENCY, RECIPROCITY, BOUNDED COMPETITION, and MEDIATION AND COMPROMISE follow in combination. Another set of micro–macro linkages occurs on the right hand side of the figure. The five micro-level factors at the lower left (i.e., starting with de-securitization) impact favorably upon INTERSTATE DEMOCRATIC DYADS.

Macro–macro connections come next. The five macro-entities at the top right listed a moment ago (i.e., starting from the far right with OBSERVANCE OF DEMOCRATIC NORMS (etc.)) have a positive effect on PACIFIC INTERSTATE DYADS.

Two factors from the environment are injected into the system at the top left of the figure: MACROHISTORICAL LEARNING and SYSTEMIC INFLUENCE

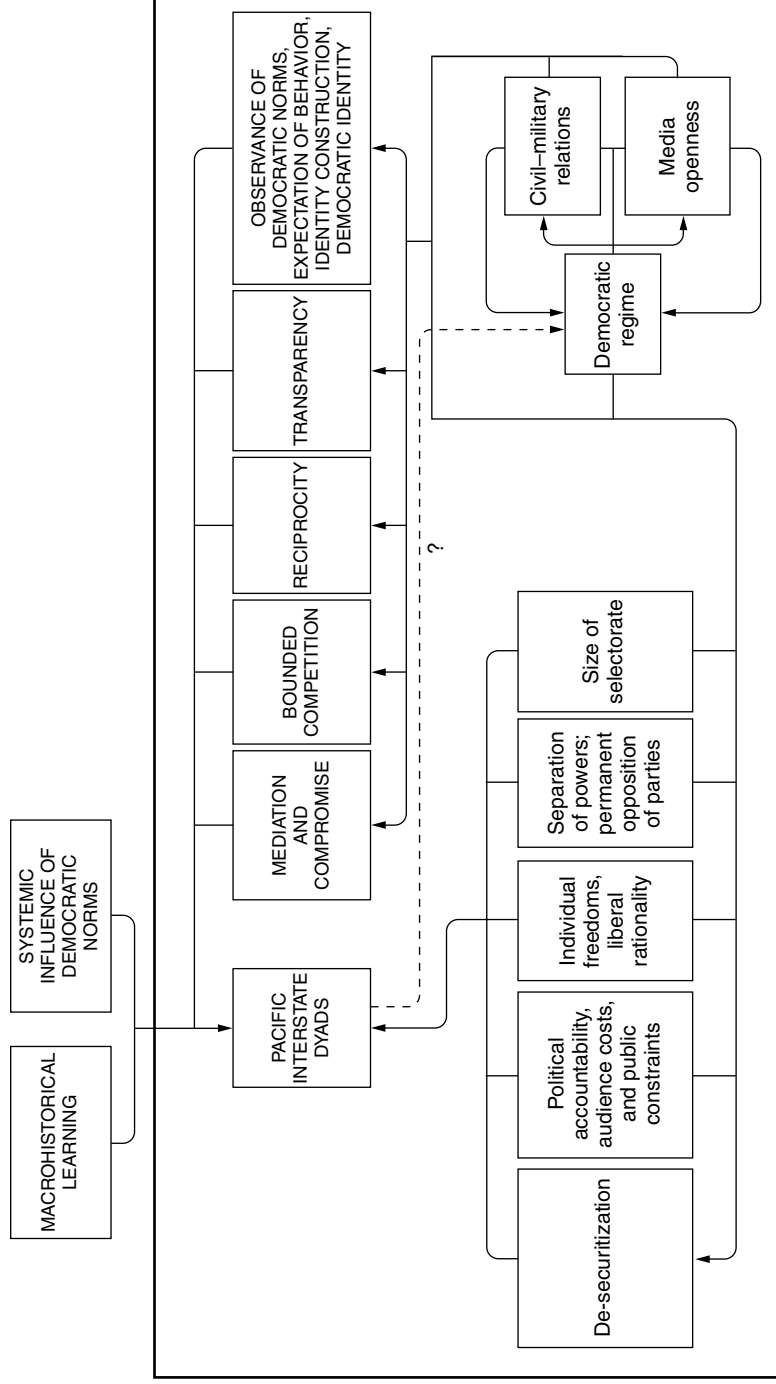


Figure 6.3 The democratic peace in 2010–2012

Sources: adapted from Chan, 2010; Hayes, 2011; and Ungerer, 2012

OF DEMOCRATIC NORMS. Each is linked favorably to PACIFIC INTER-STATE DYADS.

What about the arrow with the broken line, accompanied by an interrogation point? Its presence reflects the contested nature of reciprocal causation. As various studies point out, the idea of a positive or negative feedback from PACIFIC INTERSTATE DYADS to democratic regime is still under review.

Figure 6.3 obviously features a wide range of factors from different levels of analysis. Analytic eclecticism is obvious to see in its contents; what current *single* frame of reference – paradigm or labeled otherwise – possibly could encompass the set of causal mechanisms depicted in this figure? The answer, obviously, is “none.” The process of creating the figure serves as a reminder of the complexity of any relatively successful research program, such as the democratic peace, along with the somewhat polyglot nature of the linkages that “work” when they are put side by side with each other. Thus Figure 6.3 stands as the first implementation of analytic eclecticism in tandem with a method, namely, systemism. The result gives support to an eclectic approach toward analysis because of the obvious absence of paradigmatic purity from the explanations advanced.

Analytic eclecticism, systemism, and the way forward

This survey argues that a fusion between analytic eclecticism and systemism holds significant benefits. The two approaches compensate for each other’s weaknesses. This complementarity, in turn, provides a robust way of assessing where research programs stand today, and also fruitful directions for research. Conceptualization of human social systems in systemism – specifically, theoretical linkages within and across macro- and micro-levels of analysis and use of diagrams to permit visualization of causal mechanisms – can be of great value in clarifying relationships between contesting frameworks. Systemism, however, lacks a means for integration across multiple theories. This is where analytic eclecticism makes its contribution by providing an integrative perspective on scholarship. Yet it lacks a method for implementation, something systemism provides.

What does the new, hybrid approach contribute to the democratic peace as a program of research? Systemism and analytic eclecticism combine to illuminate the ways the field has grown in complexity as it integrates increasing numbers of social systems into explanatory frameworks and wrestles with the influence of sub- and supersystems. One example of how expositions have expanded in complexity and range of causal mechanisms would be the neo-Kantian tripod of democracy, economic exchange, and institutions put forward by Russett *et al.* (1998). This study goes far beyond a simple connection of regime-type with the degree of conflict manifested in interstate dyads.

With a combination of analytic eclecticism and systemism, our new hybrid approach allows scholars to come to terms in a concrete and systematic way with varieties of explanations and how they relate. Combination of analytic eclecticism

and systemism also gives scholars an entrée into a pragmatic mode of theorizing, focused less on covering law explanations of the democratic peace and more on ways to explore linkages between and among systems and the relevant causal mechanisms. The value of drawing attention to these linkages is greater in the context of a mature and naturally more complex research program. If the systemism-analytic eclecticism hybrid offers potentially novel roads for researchers, it has passed a significant test.

Notes

- 1 Reviews include Layne (1994), Spiro (1994), Chan (1997, 2010), Ray (2003), Rosato (2003), Chernoff (2004), Van Belle (2006), Harrison (2010), Liebel (2011), Hayes (2012), and Ungerer (2012).
- 2 On causal mechanisms as a priority, see Chan (2010: 5924) and Hayes (2012); Harrison and Mitchell (2014) address the expansion of the democratic peace into global and regional systems.
- 3 Emphasis is in the original.
- 4 Some scholars are making an effort in this regard; see Hayes and James (2014) and Cornut (2014).
- 5 Rather than introducing many more citations from the vast literature of the democratic peace, the review essays featured in this chapter will be used to identify examples.
- 6 Among other factors Ungerer (2012: 12) enumerates trade-related interdependence, international organization membership, and alliance ties and interests.
- 7 According to Sil and Katzenstein (2010: 14–15), incommensurability of paradigms is not insurmountable. Differences are (1) “less constraining when it comes to integrating elements from these theories”; and (2) “theories concerning substantive questions must ultimately rely on empirical referents to operationalize concepts, variables, and mechanisms.” Moreover, multiple theories within paradigms and variation in how terms are defined and used point in the direction of feasibility (Sil and Katzenstein, 2010: 15, 31). These observations are confirmed within the democratic peace program of research; diversity in theorizing is exemplified by structural and normative explanations and a range of concept formation and measurement is apparent regarding regime type (Chan, 2010: 5933–5934).
- 8 Systems analysis as practiced in the behavioral era of the 1960s had been dormant for decades by the time of that observation. Reasons for its demise are covered by James and James (2014).
- 9 This conclusion is anticipated in Collingwood (1946).
- 10 Pickel (2007: 397) provides a treatment of systems and mechanisms. The causal mechanisms vary with system ontology, ideational or material.
- 11 Systemism is agnostic on the long-standing debate over rational choice. Analysis based on systemism is consistent with agents acting out of self-interest but also accepts that “political action is influenced by ideological and emotional factors, too” (Bunge, 1998: 156).
- 12 The diagrammatic exposition that follows is based primarily upon James (2012; see also Bunge, 1996).
- 13 For the democratic peace program of research, later depicted in Figures 6.2 and 6.3, functional form is assumed to be incremental. The program features, collectively speaking, linear models at this stage of its development.

7 Rethinking the democratic peace

Competing accounts of “scientific progress” in IR

Ewan Harrison

There is nothing more dangerous than the deep slumber of a decided opinion.

(John Stuart Mill, *On Liberty*)

It has been over 30 years since Michael Doyle published his celebrated article in *Philosophy and Public Affairs* establishing the central claims associated with the democratic peace. His piece has formed the basis for one of political science’s most successful research programs ever. Research in the intervening period has filled in the gaps in inquiry opened by Doyle. Today, we know far more than anyone could have envisaged about the democratic peace. Yet remarkably the terms of the debate about the democratic peace basically remain the same as in the early 1980s. The empirical and theoretical arguments surrounding the democratic peace have been refined, but their parameters have not been rethought. In the intervening period, there have been extraordinary changes in world politics – changes that were literally inconceivable in 1980. The Cold War has ended. Democracy and capitalism have subsequently made enormous strides in the post-communist and the developing world. Yet in the scholarly world of IR one thing has not changed, namely, commitment to a *dyadic* peace between pairs of democratic states.

This is an appropriate moment to reflect on the trajectory of democratic peace research and the challenges the notion of the democratic peace today faces. This chapter compares three accounts of the democratic peace research program put forward by Ungerer (2012), Chernoff (2014), and Harrison (2010). Each of these accounts shares similarities. All use criteria drawn from debates in the philosophy of science to evaluate scientific progress in democratic peace research. Each agrees that research on the democratic peace provides a model of a progressive research program. However, two very different conclusions are reached about the trajectory of democratic peace research. Ungerer and Chernoff argue that research on democratic peace will be better served by continuing to stay at the dyadic level. They point to important avenues for inquiry that may still be mined at the dyadic level, and make a case for a robust consensus forming around the dyadic finding. They conclude that all is well within a vibrant and productive democratic peace research community. This account rationalizes conventional wisdom among most democratic peace researchers.

My goal is to challenge this conventional wisdom. The argument is consistent with priorities identified from the systemist point of view put forward in Chapter 6 by Hayes and James. Like Hayes and James, this chapter calls for comprehensiveness in levels of analysis as the democratic peace moves forward.

Unlike some critics of the democratic peace, who see it as a dead end, I present a constructive account of its future that utilizes recognized social scientific metrics. I agree with Ungerer's and Chernoff's insistence that democratic peace research has a promising future. However, I differ with them over how this can be realized. While research at the dyadic level has *previously* achieved an enormous amount, today it faces acute diminishing returns. Contributions to knowledge have become increasingly incremental, and this will only intensify. The dyadic claim has become "decided opinion" that is stultifying productive debate. Thus the positive story of agreement from Chernoff in Chapter 5 may have a less happy sequel when further criteria come into play.

Rather than reflecting unity and coherence within political science, consensus on the dyadic democratic peace reflects compartmentalization of research. Debates about democratic peace have become cut off from related areas of inquiry in comparative politics and political theory. In order to remain progressive in future, democratic peace research must shift to the systemic level of analysis and consider how the effects of the democratic peace vary with changes in the size and strength of the democratic community. There is nothing more dangerous for democratic peace research than continuing to "tread water" intellectually. Scientific progress is not possible to sustain over the longer term without taking risks and asking basic questions. Failure to do so will lead to stagnation, incrementalism, and growing irrelevance for the research community.

These debates are important because research resources are scarce. Scholars face choices about where to bet their time and energies. The choices academics make now will determine the future strength and vitality of democratic peace research. Furthermore, since the democratic peace research program is a paragon for political science and IR, these debates shed light on how philosophy of science debates are best utilized and applied to other areas of research. The Lakatosian (1970) metric is not just valuable in assessing previous research but in guiding the future of scholarly inquiry.

Ungerer's account of the democratic peace research program

Jameson Lee Ungerer offered an important application of criteria drawn from the philosophy of science to evaluate democratic peace research in IR (Ungerer, 2012). He uses Lakatos's MSRP to reconstruct the evolution of democratic peace inquiry (Lakatos, 1970). In doing so, he reviews seminal discussions of this issue by Ray (2003) and Chernoff (2004), as well as broader applications of Lakatosian methodology by Vasquez (1999), James (2002b), and Elman and Elman (2003). His approach to assessing democratic peace research is very similar to, and explicitly influenced by, my own approach to using Lakatosian methodology to reconstruct democratic peace research (Harrison, 2010).

Ungerer begins by discussing Lakatos's methodology and defining key elements of the democratic peace research program. Lakatos argued that research programs are comprised of three elements: a hard core, a negative heuristic, and a positive heuristic. The hard core is composed of its ontological assumptions. The negative heuristic defines the scope of the research program, setting out what evidence might be used to falsify its propositions. The positive heuristic shows how the explanatory power and content of the research program is increased while remaining compatible with the hard core and negative heuristic. Following my approach, Ungerer uses Ray's (2003) discussion of these issues to produce a more refined Lakatosian reconstruction of democratic peace research (Ungerer, 2012: 6–7). He offers a penetrating account of the evolution of the democratic peace research program that illustrates powerfully the utility of Lakatosian methodology to impose conceptual coherence upon a variety of potentially disparate ideas within democratic peace research. His approach to defining the hard core and the negative heuristic is very similar to my own (Harrison, 2010: 156–158). The hard core of the democratic peace research program consists of a series of propositions culminating in the claim that democracies behave differently than other regime types in their external relations (Ungerer, 2012: 7–9). The negative heuristic of the research program is restricted to the causal impact of democracy on the foreign policy decision-making of states (Ungerer, 2012: 9–12). However, I focus here on what Ungerer says about the positive heuristic of the research program. His account of the positive heuristic differs markedly from my mine, and will next be outlined.

Ungerer's reconstruction consists of six sets of theories (T0–T5), each building on the previous one and addressing anomalies and gaps in knowledge about the democratic peace. T0 established the basic empirical proposition and central hypotheses associated with the democratic peace. Pioneering work was conducted by the likes of Babst (1964) and Small and Singer (1976). However, this work offered no explanation of the democratic peace finding.

T1 filled this gap by showing how republican political institutions could account for the democratic peace (for example, Russett, 1993a; Gates *et al.*, 1996; Maoz and Russett, 1993; Fearon, 1995; Rousseau *et al.*, 1996). Constitutional checks and balances restrain the abilities of leaders to go to war and introduce moderation into foreign policymaking.

T2 is associated with the normative model of the democratic peace, i.e. the notion that a logic of mutual respect underpins peaceful relations between democracies (for example, Dixon, 1994; Russett, 1993a; Mousseau, 1997). This plugged a major gap in the structural account of the democratic peace by examining why democracies only appear to be more peaceful in their relations with other democracies.

T3 examined the issue of reverse causality, pioneered by Thompson (1996), Gates *et al.* (1996), James *et al.* (1999), and Mousseau and Shi (1999). This literature dealt with endogeneity and expanded the domain for democratic peace theory by showing that peace can also be a cause of democratization (Ungerer, 2012: 19).

T4 introduced the selectorate-based institutional explanations of the democratic peace. This approach became very fashionable from the 2000s onwards and was developed by Lake (1992), Bueno De Mesquita *et al.* (1999), Reiter and Stam (2003), and Gelpi and Griesdorf (2001). T4 offered a revised account of the institutional argument and produced novel predictions about the relevance of regime-type in conflict. It generated the proposition that democracies are better able to win the wars that they choose to fight due to the nature of their domestic political makeup and the incentives created for rulers by their political institutions.

T5 of the democratic peace research program examined the idea of economic causes of democratic peace. This approach has been associated with Gartzke (2007) and Mousseau (2000). It asserts that economic norms are crucial and that capitalist states (Gartzke) or those that have contract-intensive economies (Mousseau) mutually cooperate (Ungerer, 2012: 23).

Ungerer therefore claims that there have been five “intra-program shifts” (2012: 23), each compatible with the hard core and negative heuristic of the research program, which demonstrates expanding explanatory power. This is the hallmark of scientific progress. Furthermore, Ungerer considers the implications of these contributions not just for the previous development of democratic peace research, but for its future. He claims that of all the theories, two were the most progressive. These two theories are T3 (reverse causality) and T5 (economic norms). These

epitomize the two most promising arenas of future research. With limited resources ... scholars would be advised to address these ... to ... increase the explanatory power of the theories and the policy implications that can be derived from them or present “falsifying evidence” that enables the IR community to eliminate some potential causal mechanisms allowing the focus to be shifted to others.

(2012: 25)

However, the dyadic finding is common to all five elements of the positive heuristic. Hence, to Ungerer, the future of democratic peace research is to continue down the dyadic route.

Chernoff’s account of the democratic peace research program

Chernoff (2014) offers another key reconstruction of the democratic peace research program. Like Ungerer, he couches his assessment of democratic peace research in terms of philosophy of science. However, while influenced by Lakatos as well as others, the primary point of reference for Chernoff’s evaluation is the work of Pierre Duhem. Chernoff focuses on Duhem’s “approach to consensus” as the mechanism for identifying scientific progress. Different scholars, and the scientific research community, must be persuaded of the validity

of an empirical finding and its explanation. If this can be achieved, scientific progress can be spoken of. If it cannot, then scientific progress is called into question. If skeptics of a proposal can be convinced about its truth or at least cease to question it publicly, then they may be said to have been persuaded of the truth and validity of a claim. Chernoff's argument represents the product of years of writing about these issues (Chernoff, 2005, 2007a), and he pursues his position with a tenacity that is compelling. His work is bound to make a major impact, making it particularly important to address.

Following Kuhn, Chernoff's starting point is descriptive. He documents which studies have been influential in three prominent research programs in IR: alliance formation, nuclear proliferation, and democratic peace. He utilizes two approaches to identify the most influential studies in the three subfields. The first consists of surveys of graduate syllabi from prominent research institutions. The second approach is provided by the opinions of journal editors of the major journals in the field. Chernoff then identifies a list of the ten most important studies in each of the three subfields he identifies. This descriptive exercise is a fascinating contribution in itself, and the data he offers will surely provide a focal point for much further discussion. His aim in carrying out this exercise is, however, not descriptive. It is to identify the extent to which any approach to consensus exists among scholars within each of the subfields.

Applying this methodology to democratic peace research, Chernoff identifies ten articles as having the greatest influence. It is important to recognize that Chernoff is *not* arguing that these works reflect the preferences of particular scholars, but rather the aggregate preferences of the security studies research community. Interestingly, his account of the most prominent works in the democratic peace research program overlaps significantly with the account offered by Ungerer. The major works that Chernoff identifies are: Small and Singer's influential qualitative analysis (1976); Rummel's five-volume study (1975–1981); Doyle's original article (1983a); Mearsheimer's "Back to the Future" critique (1990); Bremer's studies of the war proneness of different sorts of dyads (1992); Lake's (1992) assessment of the war fighting capabilities of democracies; Maoz and Russett's classic study of the structural and normative causes of the democratic peace (1993); Owen's (1994) analysis of the role of perceptions in causing the democratic peace; Layne's (1994) case study critique of the causal veracity of the democratic peace argument; Spiro's statistical critique of the democratic peace (1994); Farber and Gowa's (1995) influential extension; and Gartzke's work on capitalist peace (2007). According to Chernoff, these ten points of reference provide us with a reliable guide to the "state of the art" on the democratic peace.

From this survey, Chernoff extrapolates two important conclusions. His first is that there "has been a significant degree of approach to consensus in the democratic peace debate with scholars accepting that liberal explanations for dyadic behavior are superior to the realist explanations" (Chernoff, 2014: 233). He points out that none of the major journals in security studies have published a single article challenging the dyadic hypothesis since 2000 (2014: 229–230).

The best available explanation of this consensus, Chernoff concludes, is that the scholarly community has been convinced of the validity of the liberal argument. While individual scholars might continue to protest this consensus, they do not reflect the overwhelming view of the field. (Of course, critical scholars such as Ish-Shalom or Sjoberg, in line with their arguments from Chapters 8 and 9, might note that dissenting views from outside of neopositivism largely are excluded from the debate throughout its existence.) Chernoff's second conclusion follows from his first. It is that in

recent years the democratic peace debate has shifted to a focus on questions that assume the superiority of liberal explanations for democracies dyadic behavior, seeking the best explanation among various particular aspects of liberal states, such as liberal trade, structure or norms.

(2014: 236)

The liberal argument has prevailed, and debates are about which liberal hypothesis is best.

Chernoff does not stop there. With the logical veracity of a philosophically trained scholar, he underscores the significance of this finding by comparing the consensus which exists in relation to the democratic peace with the other two areas of inquiry. He finds a stark contrast between the consensus achieved over the democratic peace with what exists in relation to debates about nuclear proliferation and alliance formation. In the area of nuclear proliferation Chernoff identifies little if any overlap between realist and alternative explanations. In relation to alliance formation, things are slightly better. Commentators are roughly evenly split with respect to whether balancing or bandwagoning is the primary pattern and whether power or threat is the key variable. This is a higher level of consensus than in the debate surrounding proliferation. However, especially since 2000, no overall consensus has emerged (2014: 175–177). Hence, the approach to consensus that exists in relation to the democratic peace stands head and shoulders above debates in rival research programs. Chernoff concludes that this is a remarkable achievement and infers that scientific progress has been achieved. A consensus has been reached about the democratic peace that has enabled debates to move from broad-based discussions to more fine-grained causal explanations.

As with Ungerer's argument, on its own terms Chernoff's reconceptualization of the democratic peace research program is compelling. Chernoff's work is particularly insightful and illuminating since his book-length study offers the opportunity to (1) systematically survey the state of the art in a way in which has not been done before; and (2) situate democratic peace research in comparative perspective to other subfields. As an intellectual achievement and broader empirical and theoretical endeavor, Chernoff's approach is impressive. Nevertheless, Chernoff buys into the exact same parameters that Ungerer's analysis accepts. He accepts the dyadic terms of debate. This leaves open the possibility of an alternative account of scientific evolution within the democratic peace research

program. It is this line of inquiry which I have developed (Harrison, 2010), and which I revisit here in the light of the contributions by Ungerer and Chernoff.

An alternative approach: shifting to the system level

Elsewhere I have offered a third reconsideration of debates about the democratic peace using criteria drawn from the philosophy of science (Harrison, 2010). Here I compare that approach to those offered by both Ungerer and Chernoff. Like Ungerer and Chernoff, I argue that scientific progress in IR has been achieved in democratic peace research. I also agree that this can be sustained into the future. Where I differ from them is over how best to understand and sustain this momentum. My key point of contention is that the *system* level rather than the dyadic level provides the best route forward for democratic peace research. My approach will be compared first to Ungerer's and then to Chernoff's with a view to highlighting both its similarities and points of difference with them.

Like Ungerer, I focus on the relevance of Lakatosian methodology of scientific research programs for evaluating democratic peace research. Moreover, as discussed, my definitions of the hard core and negative heuristic of the democratic peace research program are sufficiently similar to those offered by Ungerer that this need not be dwelled on. Indeed, this similarity between Ungerer's and my approach is unsurprising, since Ungerer drew explicitly on my definitions of the hard core and negative heuristic when formulating his own analysis. It is over our respective accounts of the positive heuristic of the democratic peace research program that differences of emphasis arise. In particular, in contrast to Ungerer, my account is based on the notion of a shift to the systemic level of analysis.

I argue that the democratic peace research program can be viewed as a progression of theories (monadic, dyadic, systemic) unfolding logically and cumulatively from a set of axiomatic assumptions that comprise its hard core and negative heuristic. Monadic theories have been subsumed and superseded by dyadic theories, which are now being superseded by systemic theories. A second difference between Ungerer's approach and mine is that I adopt a broader and more pluralistic methodological take on the democratic peace research program. My approach self-consciously incorporates the contributions of constructivist and non-positivist scholars such as Wendt (1999), Oren (1996), and Williams (2001). My argument is that Lakatosian methodology might be useful in getting *both* positivist and non-positivist scholars interested in the impact of democracy on international affairs talking to one another. Ungerer's approach focuses more exclusively on positivist literature.

Like Ungerer, I reconstruct the positive heuristic of the democratic peace research program in a series of clusters. I identify four clusters of democratic peace research. The first (or "T1," to adapt Ungerer's terminology) examines the central theoretical and empirical claims about the democratic peace.¹ Here systemic approaches have proposed that the effects of the democratic peace might vary over time and with the changing strength of the democratic community.

The democratic pacific union might produce spillover which facilitates the holistic spread of the zone of democratic peace. The dyadic claim limits the pacific union to a “separate peace.” However, if the pacific union produces spillover, then this might affect patterns of peace and conflict in the international system as a whole. Two approaches have been put forward. One is that once a critical mass of democracies emerges, the democratic peace might generate “socialization” or “network” effects that cause it to spread (Mitchell, 2002; Harrison, 2002, 2004b; Kadera *et al.*, 2003; Snyder, 2013a and b). The other postulates waves or cycles in which increased progress or temporary reversals can occur (Cederman, 2001a). Systemic perspectives envisage an endogenous process pushing the international system down a certain path. Thus Huntley (1996) and Harrison (2002) note that the collapse of the Soviet Union might be explained by systemic cultural selection working against highly authoritarian states. Analyses of when democratic waves have occurred also have been offered, with three key dates being debated as thresholds – 1919, 1945, and 1989 (Mitchell *et al.*, 1999; Green, 1999; Ikenberry, 2001).

The T1 cluster is capable of encompassing both institutional and normative models of the democratic peace. Mitchell *et al.* (1999) use rationalist insights to model selection logics favoring democracy. They reconcile apparently contradictory hypotheses that both peace and war can be causes of democracy, and offer a synthesis that links peace, war, and democratization within an evolutionary model. Until a threshold level, war prompted democratization because democracies tend to win the wars they fight and states that lose wars often shift in a democratic direction. Afterwards, however, beyond the threshold level, democratization decreases the amount of war in the system, and after this point peace becomes the major cause of the spread of democracy.

Wendt (1999) offers an account of democratic socialization compatible with a normative logic. Once a critical mass of democracies exists, a Kantian culture emerges as the dominant systemic tendency. This results in socialization pressure causing the spread of the democratic zone of peace (Mitchell, 2002; Harrison, 2004a). Thus both rationalists and constructivists can address systemic hypotheses, and their approaches should be viewed as complementary (Fearon and Wendt, 2002; Harrison and Mitchell, 2007). Various mechanisms might be envisaged through which these effects operate, including spillover, competition, socialization, positive feedback and virtuous cycles, snowballing, and reflexivity or situations in which knowledge about the spread of democracy itself becomes a major cause of the spread of democracy. The intermediate conclusion here would be that it is time for the democratic peace to move beyond a dyadic “separate peace” formation. However, this is to be achieved without resorting to the proposition that there is a monadic relationship between democratic and peace. Instead, this approach is careful to stipulate the conditions under which the democratic community might generate symbiotic effects (Harrison, 2002, 2004a; Snyder, 2013a and 2013b).

T2 examines empirical evidence for and against the democratic peace. Cederman (2001a) has proposed that the democratic peace should not be understood as

a time invariant law but as an evolutionary macro-historical trend. This alters the way in which we should approach empirical evidence in the debate over the democratic peace. Thus nineteenth- and twentieth-century “near miss” international crises may corroborate the democratic peace finding because the pacific union was in the early stages of development. This encourages empirical researchers to look for “maturity effects” in the development of the democratic peace, in which democratic, mixed, and even autocratic dyads may exhibit more peaceful relations over time as the democratic community gradually consolidates. Moreover, it also encourages researchers to factor into their analyses “epochal effects” or the way in which temporary reversals or setbacks in the global spread of democracy may be part of a global trend toward its eventual consolidation. Thus the democratic reversals and setbacks of the interwar period laid the foundation for a second wave of democratization after World War II. Equally, Gartzke and Weisiger (2014) have pointed out that empirical skepticism about the democratic peace may operate at the systemic as well as the dyadic level of analysis. They argue that systemic researchers on the democratic peace have failed to control for variables that trend with economic development in the international system.

Another important area for debate in this second cluster is Gartzke and Weisiger’s argument (2013a and 2013b) that systemic change may generate malign effects. Thus there might be reason to expect that inter-democratic conflicts will proliferate in a world with a strong democratic community. The size and relative strength of the democratic community will make conflict between democracies more likely and increase the possibility that democratic solidarity will break down because of the relative weakness of the autocratic community. Hence the dyadic democratic peace finding was the temporary byproduct of a world with a weak and small democratic community, and history will begin “all over again” as democracy becomes commonplace. Dyadic theorists have been skeptical about this analysis empirically (Dafoe *et al.*, 2013), but in the face of the growing salience of inter-democratic disputes in international relations (Iraq 2003, the euro crisis, the Snowden affair, and Syria 2013) this critique seems implausible. By contrast, Mitchell and Harrison have launched a counter-response to Gartzke and Weisiger from a systemic perspective (Harrison and Mitchell, 2014). Adopting a systemic perspective we accept Gartzke and Weisiger’s basic theoretical position about inter-democratic conflict in a world with a strong democratic community. However, we propose that inter-democratic conflicts will not be fundamental enough to stop the spread of democracy. Thus inter-democratic conflicts will become more salient even as the democratic community expands – a counterintuitive pattern. Mitchell and I extrapolate that a major locus of conflict in world politics in the future may be a “clash of democratizations” between the old and the new democracies as they seek to gain the benefits and recognition associated with the previously exclusive democratic gentleman’s club.

T3 examines endogeneity between peace and democracy. Here I locate research identifying the existence of positive feedback loops between peace and

democracy. Peace may cause democracy, but democracy causes peace, thus generating a virtuous cycle (Thompson and Rasler, 2005). Russett and Oneal put forward a related idea of the Kantian triangle – positive and mutually reinforcing relations that exist between democracy, interdependence, and international institutions (Russett and Oneal, 2001). A further possibility includes norm cascades. When a critical mass of democracies exists this might generate effects which are worth more than the sum of their parts (Mitchell, 2002). A positive feedback cycle may become locked into place and gather momentum over time. This might work to intensify reflexivity and encourage snowballing of democratization. A further line of inquiry is the mutually reinforcing nature of change at the international and transnational levels. There has long been a gulf in IR between these two approaches, with major commentators such as Wendt ultimately being forced to concede that norm dynamics operate purely at the inter-state level (Wendt, 1999). However, transnational and international dynamics have the potential to be mutually reinforcing. Transnational processes and the expansion of social and economic interactions across borders may reinforce the incentives and constraints generated by the strength of the democratic community in the system of states to produce a mutually reinforcing cycle (Snyder, 2013a and 2013b; Harrison and Mitchell, 2014).

Feedback loops examined by the T3 cluster may operate at regional as well as global levels. Regional forces can magnify global ones within specific geographic contexts (Gleditsch, 2002). A strong global democratic community has regional democratic communities within which democratic spillover is intensified. The expansion of regional democratic communities strengthens the global democratic community. Strong connections between societies and governments enhance regional integration and democratic spillover. These connections are likely to be greater between close or contiguous states. High levels of both linkage (the density of elite or societal ties between two countries) and leverage (the vulnerability of a country to outside pressure) encourage democratization (Levitsky and Way, 2010: 41–45). Additionally, countries that experience stable democratic transitions might increase the likelihood that their neighbors follow the same path (Starr and Lindborg, 2003; Kozhemiakin, 1998; Gleditsch and Ward, 2000). This creates a “neighbor emulation effect,” encouraging countries to converge around democracy within specific geographic clusters. This regional convergence can occur in situations where there are large differences in levels of democracy (e.g., East and West Germany) (Brinks and Coppedge, 2006). Yet it may also occur between countries which are similar and densely connected at societal and elite levels (e.g., Mexico and the United States). Regional dynamics such as these hold out the possibility of a “peace in parts” facilitating a larger global pattern (Keohane, 2003).

Finally, T4 focuses on the relationship between democratization and war. Kadera *et al.* (2003) show that war is less likely to cause democratic failures, especially for new democracies, when the global democratic community is strong. Having a strong global democratic community protects fledgling democracies. Gates *et al.* develop this model by refining Huntington’s thesis about

waves of democratization (Gates *et al.*, 2004; Huntington, 1991). They identify that the peaks of democratic waves are related to the two world wars and the end of the Cold War. Even if some new democracies backslide, in the aggregate there is a net democratic consolidation with each new wave. For example, much has been made of the “failure” of the Arab Spring. Yet if Tunisia makes a stable democratic transition, democracy will have gained an important foothold in a region where it was previously absent (Harrison and Mitchell, 2014). Finally, a strong global and/or regional proportion of democracies increase the chances of democratic survival. A “stepped” pattern emerges in which there is an aggregate trend toward democracy. Thus the argument that democratization can encourage nationalist conflict is true, but is part of a larger trend toward overall spread of democracy. This is compatible with Kozhemiakin’s (1998) important and unduly neglected critique of Mansfield and Snyder’s (1995a) argument. Kozhemiakin holds that unstable and aggressive democratic transitions constitute only one pathway among several that states can pursue. An alternative pathway is the model of democratic transition, such as that pursued by, say, Poland, South Korea, or Brazil, in which states voluntarily and peacefully join up to the democratic community (Kozhemiakin, 1998). This is consistent with the social contractarian model of the extension of the democratic community offered by Wendt (1999) and Rawls (1999).

Overall, I argue that a “second generation” of democratic peace research has begun to emerge which operates at the system level.² My account maps a markedly different future trajectory for democratic peace to that offered by Ungerer. Two salient points arise from this comparison of Ungerer’s and my approaches. First, an obvious question emerges about the “objectivity” of the metric provided by Lakatosian methodology. How is it that two scholars using the same supposedly independent yardstick provided by Lakatosian methodology reach such different conclusions from essentially the same premises – including similar definitions of both the hard core and negative heuristic of the research program? Moreover, what does this tell us about the use of philosophy of science criteria in the social sciences? Surely if Lakatosian methodology were genuinely objective, different scholars working independently would reach the same conclusions and prognoses for democratic peace research? It is not clear where this leaves Lakatosian methodology as an “objective” criterion for theory evaluation in the social sciences. A further question relates to the extent to which Lakatosian methodology can be useful to us not just as a guide to evaluating previous research, but in evaluating choices about which questions to research *in future*. Ungerer notes that the best bet for researchers is a continued focus on the most successful aspects of dyadic research. By contrast, I argue in favor of the system level. These are two radically different accounts of where researchers might best invest their scarce research resources.

Equally, my analysis has very different implications from that of Chernoff in terms of the future trajectory for democratic peace research. Chernoff’s argument is that a major achievement of democratic peace research has been an “approach to consensus” and the progressive narrowing of questions being

pursued by inquiry. My approach, however, implies the opposite. What Chernoff interprets as an approach to consensus is to me a “lazy consensus.” By staying at the dyadic level of analysis after 30 years of inquiry, diminishing returns to investment have started to creep in to democratic peace research. While contributions at the dyadic level of analysis were previously large and non-incremental, this has shifted. Democratic peace research is now in danger of crossing a “boredom threshold” in which very similar terrain is covered by researchers who are asking increasingly incremental questions. Instead of continuing down this path, I argue that democratic peace researchers would be better off asking fundamental questions about the basic parameters of the phenomenon they are investigating. This will open up scope for non-incremental contributions to be made. It is by making bold conjectures that the most important breakthroughs in scientific knowledge have tended to be made. Doing so has the potential to produce a “second generation” of democratic peace research, and in so doing provide the intellectual foundations for another 30 years of productive inquiry in this subfield. This position also meshes with recent calls for a return to grand theory in the study of IR (Mearsheimer and Walt, 2013; Snyder, 2014; Harrison and Mitchell, 2014).

One response that Chernoff might make to my proposed shift to the system level is that it has not yet been confirmed by testing. Unless systemic democratic peace literature is able to persuade the profession that it is superior to dyadic accounts of the democratic peace, then it cannot claim to be scientifically superior. In other words, Chernoff might argue that systemic democratic peace research has not yet achieved the approach to consensus that dyadic research has. However, three responses might be made to any such argument. First, systemic approaches to the democratic peace have not been subjected to sufficient testing. A fair competition between systemic and dyadic hypotheses has in this way not taken place. Second, on the limited number of occasions when systemic hypotheses have been tested, considerable supporting evidence for them has been found. This indicates that there are rich pastures for inquiry which have barely even begun to be pursued by democratic peace researchers.

Finally, it is also possible to question Chernoff’s argument that the democratic peace has become a focal point for an “approach to consensus” within political science. Indeed, the opposite might be true – that the “cottage industry” surrounding the democratic peace research program reflects the Balkanization and fragmentation of the field into “sub-subfields” that are not cross fertilizing. Over the last 25 years democracy has spread on a global scale. Given this development, an outsider to the profession might have been forgiven for thinking that a major focus for research within IR would be on the global spread of democracy. One might also have assumed that this literature would benefit by and draw upon the literature on the international dimensions of democratization in comparative politics to explain the potential similarities and complementarities these disparate bodies of literature might offer (Huntington, 1991; Whitehead, 1991). Equally one might assume that there would be extensive cross fertilization between the literature on state socialization and the diffusion of norms and

the democratic peace. In fact, however, modern democratic peace theory in IR finds itself almost completely at arm's length from these literatures, there being little or no meaningful interchange between them. Mearsheimer and Walt's (2013) arguments about the impact of professionalization and competition explain why. As the domain and significance of findings is narrowed down, so it becomes easier to make empirical arguments which are hard to challenge and can survive peer review while having no impact outside the narrow subfield. In other words, the democratic peace literature has become a sealed-off peer community that speaks its own self-referential language. This reflects not the ability of academics to speak to one another, but rather the growing tendency of professionalization and specialization within the field to produce "turf wars" between groups of scholars.

These arguments equally apply to insights from political theory. Consider John Rawls's *The Law of Peoples*. This book has been virtually ignored by empirical researchers in political science, despite the fact that it engages in a wholesale fashion with the dyadic democratic peace literature in IR (see also Ish-Shalom in Chapter 8). As I have argued elsewhere, this represents a lost opportunity for democratic peace theory. Due to his operation within the social contract tradition and given his focus on the normative expansion of the pacific union, there is enormous scope to link Rawls's account of the democratic peace to systemic analysis of the spread of the democratic peace (Harrison and Mitchell, 2014). Yet this has not occurred because empirical scholars of the democratic peace have not generally taken his work as making a serious contribution to the empirical analysis of the democratic peace.

Equally, the importance of Fukuyama's "End of History" argument has also been underappreciated in the democratic peace literature. As with Rawls, the prevailing view among democratic peace researchers seems to be that Fukuyama did not make "empirical claims" that might be subject to testing (Fukuyama, 1992). Yet this seems perverse since manifestly at the core of Fukuyama's argument was precisely an empirical claim about the gradual spread of democracy. More generally, the writings of Kant, Hegel, Hume, Smith, de Tocqueville, and Mill all powerfully anticipate key themes in world politics at the start of the twenty-first century as democracy and capitalism take root worldwide. The literature of the democratic peace would be wise to tap into the classic insights offered by these scholars and use them to understand contemporary developments (Harrison and Mitchell, 2014).

Conclusions

In the spirit of John Stuart Mill's observation that there is nothing more dangerous than the deeper slumber of a decided opinion, this chapter has aimed to "upset the apple cart" of conventional thinking about the democratic peace. Thirty years after the publication of Doyle's seminal article in *Philosophy and Public Affairs*, democratic peace research today faces a fork in the road. Conventional wisdom has it that the best way for democratic peace research to proceed is to continue to develop and provide more fine-grained analyses at the

dyadic level. This conventional wisdom is justified with considerable tenacity and logic, by both Ungerer and Chernoff. By contrast I have taken the opposite view. I agree with Ungerer and Chernoff that criteria from debates in the philosophy of science can be extremely illuminating in thinking about the past and future trajectory of democratic peace research. I also agree with them that these criteria should explicitly inform our decisions about where to deploy our scarce research resources. In both of these areas, critics of the democratic peace are mistaken. However, I disagree with Ungerer and Chernoff on how best to understand the future trajectory of democratic peace research using these criteria.

Contrary to Ungerer and Chernoff, the argument of this chapter has been that the dyadic democratic peace finding is in danger of becoming, and perhaps already is, exactly the kind of “decided opinion” and unquestioned orthodoxy that Mill was concerned about. By accepting limited scope conditions and narrowing its hypotheses to address increasingly incremental contributions about world politics, the democratic peace literature is in danger of putting itself out of business. Rather than representing an “approach to consensus,” the democratic peace has become a lazy consensus, something that is rather too easy and safe to buy into. Yet this comes at the price of sealing democratic peace theory off from other subfields in political science which it would benefit from interchange with, and has created an increasingly self-referential cottage industry of researchers offering findings of little broader or general validity for IR.

An alternative way forward for the democratic peace research program is for it to broaden its scope and ambition. This involves moving to the system level by examining how the effects of the democratic pacific union might vary over time and with the strength of the democratic community. This involves taking some risks, but continued operation at the dyadic level also comes with risks – namely the risks of stagnation and increasing incrementalism. This is in line with the systemist point of view articulated in Chapter 6 by Hayes and James because the dyadic democratic peace is inherently reductionist and therefore self-limiting. By contrast, moving to the system level opens up the prospect of rejuvenating democratic peace research by greatly increasing its scope and significance. Thus Ungerer and Chernoff both actually *underestimate* the utility of philosophy of science criteria in guiding the future of democratic peace research into uncharted waters. If democratic peace researchers are afraid to set sail on the high seas of the system level, they risk forever remaining on the sidelines of the most important debates about the evolution of world politics taken as a whole.

Notes

- 1 I will start with T1, not T0 as Ungerer does, because this fits with the approach I have previously adopted (see Harrison, 2010, where I used the terminology “PH” for “Positive Heuristic” 1–4).
- 2 By “generation” I mean a period of 25–30 years, i.e., the length of time democratic peace inquiry has been around. I do not consider monadic approaches to constitute a generation for research since the monadic level was largely ruled out after the first few years of democratic peace inquiry.

8 The normative within the explanatory

A critical take on the democratic peace literature

Piki Ish-Shalom

Introduction

There is something very alluring and tempting about positivism and objectivity. We conduct our research as if behind the veil of ignorance. We shed our personalities and leave them behind us to conduct our studies, our theorizing. A researcher, the positivist argument goes, is the scientific equivalent of Rawls's unencumbered self.¹ Our personal traits and creeds are of no importance and we view our subjects of studies from a nowhere point. Academic analysis is a detached conduct and anyone (at least anyone intelligent and trained enough) could step into our observatories, see exactly the same things, collect the same data, and reach the same conclusions. This point of view is associated with self-perception of neopositivism as described in a highly critical way by Jackson in Chapter 1. On one level the quasi-hegemony of neopositivism is disappointing, as it removes any aura of creativity. Yet the assumed objective technicality allows a moral detachment, and absolves us of the obligation to ask about moral responsibilities which might be involved in social research.

Our involvement in the world, from the preceding point of view, is an indirect one. For example, by detachedly studying war we may find the sources for, and the forces operating in, war. The knowledge produced can be acquired by decision makers and used to devise ways to better cope with war; better in the sense that they can try to solve war as a social problem or that they can try to develop strategies to win wars, or even use war as a political instrument, as in the rally-round-the-flag effect. Theorists, as producers of the information, can remain aloof in their ivory towers as to the uses of the theories. This indirectness absolves theorists from moral responsibilities. Theorists are not the agents of war, only detached observers.

But like many tempting beliefs, (neo)positivism and objectivity do not stand the trial of analytical scrutiny or moral reasoning. Research is not done beyond the veil of ignorance from some nowhere observatory by unencumbered selves. Research is conducted by flesh and blood individuals, who have personal histories, socioeconomic standpoints, moral commitments, and sectoral interests. At least some of these traits cannot but inform the process of analysis. Moreover, theorists are not external observers of the social reality they study. By producing

knowledge and theoretical insights they are involved in forming, shaping, and constructing political events and social processes. Giddens asserts this bind by his double hermeneutics, which he defines as “a mutual interpretative interplay between social science and those whose activities compose its subject matter” (1984: xxxii). Theorists are a part of society and they are producers of valuable knowledge; knowledge that is both value-laden and has the potential of being translated into actual policies.

The moral groundwork of theories, and responsibilities theorists are endowed with, is evident in the democratic peace theories, which made their first step in 1964 with the publication of the neglected – and even now somewhat obscure – article by Dean Babst (1964). It took two additional decades before democratic peace made its real impact. Babst’s theoretical observations were revived during the 1980s when a growing number of researchers reaffirmed an interesting empirical phenomenon – that democracies rarely go to war with each other. The observation steadily gained support and a thesis emerged according to which democracies do not fight each other because they are democracies. This was followed by a further theoretical phase which attempted to explain the phenomenon and to identify those characteristics of democracy which produce peace. The theories that tried to explain the phenomenon and the thesis crystallized into two theoretical families. The first explains the democratic peace phenomenon by focusing on the structural dimensions of democracy, and the second family of theories highlights the normative dimensions of democratic societies.

It will be argued that the debate between these two families of theories, the structural and the normative-cultural, is a debate over explanation, but that debates over explanations are also moral deliberations. It will be further argued that the constitutive relation between the explanatory and the normative is grounded in the essential contested nature of the concepts of democracy, war, and peace. Bearing in mind the moral groundwork of theories and the essential contestedness of concepts, I propose the epistemological and methodological strategy of *Zooming In Zooming Out*. This is a strategy by which theorists zoom into the internal components of their theories, namely the concepts, and at the same time define and conceptualize them with moral sensitivity. *Zooming In* asks theorists to focus their theoretical rigor on better defining the concepts they use. *Zooming Out* adds normativity to the commonly accepted criteria of exhaustiveness, exclusiveness, and operationalization. It asks theorists to be ready to justify morally the definitions with which they operationalize the concepts they use. To meet that standard, theorists must reflexively and critically engage with the moral commitments that inform their theoretical work. *Zooming Out* also calls on them to morally judge the real-world ramifications of their theories. All of the preceding is intended to connect favorably with the concept of sociable pluralism, articulated by Freyberg-Inan in Chapter 4, which provides the unifying concept for this volume.

The concepts of war, peace, and democracy

The basic constituents and building blocks of theory are concepts; thus one of the first tasks in theorizing is defining the relevant concepts. However, defining concepts is not as straightforward as sometimes assumed, as concepts used in the social sciences are essentially contested. Being essentially contested implies having several existing meanings of a concept, at least some of which are reasonable and legitimate, and all of which are grounded within a normative framework. Take peace, war, and democracy. Peace is understood differently in the various theories of IR. The realist conceptualization of peace is the absence of war (Waltz, 1959: 1). For realists, this absence is temporary; peace is but a break between wars.

If peace is defined by the absence of war, then war is also a fundamental concept of democratic peace theories. War further becomes a subject to contested conceptualizations, as it too has several definitions and explanations. For instance, Brown defines international war as “violence between organized political entities claiming to be sovereign nations” (Brown, 1994: 1). Clearly, this definition contains other contested concepts, such as “violence,” “political,” “nations,” and “sovereignty.” We are seized in an unending conceptual path, a need for defining increasingly derivative concepts.

And the realist conceptualization of peace is not the only one. Boulding (1979) would see this realist view of peace as unstable, even immoral. Real peace equals “stable peace” – “a situation in which the probability of war is so small that it does not really enter into the calculations of any of the people involved” (Boulding, 1979: 13). This idea is different from the realist conceptualization of peace as the absence of war. The alternative is a peace that would not easily slip into war – a moral peace with normative and material dividends. When stable peace of this kind exists, as it does between the United States and Canada, few resources are invested to ensure military protection against the stable peace partner and a security community can be achieved, one in which resources can be used to enhance justice and prosperity. Yet also this conceptualization implicates many more derivative concepts, such as interests, calculations, rationality, or legitimacy. Deutsch’s idea of the “security community” offers another plausible explanation of stable peace (Deutsch *et al.*, 1957), which invokes a common identity. We are caught again on an unending conceptual and morally laden path.

The third concept – democracy – is another definitional minefield. Gallie (1956) used democracy in his seminal article as a paradigmatic example of an essentially contested concept. Two broad paradigms of democratic theory exist. The first paradigm is elitist, structural, formal, and procedural. It tends to understand democracy in a minimalist way (see, for example, Schumpeter, 1962; Lippmann, 1955; Przeworski, 1999). A regime is a democracy when it passes a certain structural threshold and has free and open elections; autonomous branches of government; division of power; and checks and balances. This precludes a tyrannical concentration of power in an elite.

The second paradigm, which relates to normative, cultural, deliberative, and participatory democracy,² focuses on other issues beside structure and demands much more of democracy (see, for example, Pateman, 1970; Mansbridge, 1970; Barber, 1984; de-Shalit, 1997; Elster, 1998; Habermas, 1998; Dryzek, 2002). First, it emphasizes society and citizens – not the political system and the regime. Second, it demands democratic norms and culture. This implies political rights, tolerance, openness, participation, and a sense of civic responsibility.

Choosing between the two paradigms means choosing between two normative paths, since the paradigms stem from different worldviews. The first is conservative; the second is liberal. The minimalist structural conceptualization of democracy stems from conservative skepticism regarding human faculties. According to this view, it is not rationality that drives human action, but instead desires, instincts, and communal traditions. These are extra-rational and drive humans to strive for power. There are two major consequences to this: first, because everyone seeks power, social and political organizations are at perpetual risk of destabilization. Second, there is the opposite consequence, the constant danger of a dictatorial concentration of power in the hands of those individuals who succeed in gaining power. The conservative solution to these two dangers is minimal, structural democracy. Through regular elections, democracy guarantees that no power will last forever and prevents the concentration of power. Yet by confining political participation to elections, democracy precludes political and social destabilization.

The normative and cultural conceptualization of democracy is based on an optimistic, liberal view of rationality. Humans are rationally driven. While they possess emotions, desires, instincts, or communal bonds, these are subject to rational calculations, including controlled political behavior. The rational individual is also seen as the locus of indivisible civic rights. This normative and cultural definition of democracy centers on participation and rights, seeking to widen the scope of citizens' political participation, and thus extend the meaning of democracy. According to this view, there is little fear of destabilizing the polity because political participation is rationally based.

Peace, war, and democracy – the core concepts in democratic peace theories – are essentially contested. We next consider how this contestedness affects the content of theories.

The different democratic peace theories

The essential contestedness and normative content of the involved concepts translate to explanatory theories – those higher order idea entities that are structured by the concepts. What follows will focus on articles that typify the two paradigms of democratic peace and show how each is founded on a specific normative understanding of war, peace, and especially of democracy.

During the 1980s, a number of articles on democratic peace were published (Doyle, 1983a, 1983b, 1986; Rummel, 1981, 1983), but the “big bang” arguably occurred in 1993. In an oft-quoted article, Maoz and Russett (1993) injected the

democratic peace into the lifeblood of IR. The article contributed greatly to the recognition of two distinct families of theory, offering rival explanations of the democratic peace – normative and structural. Maoz and Russett asserted the empirical observation of democratic peace (1993: 624). Yet they acknowledged the disagreement regarding its cause. The task Maoz and Russett set was to decide which of the accounts, the normative or the structural, is the stronger.

According to Maoz and Russett, the normative theory is based on two assumptions: (1) “States, to the extent possible, externalize the norms of behavior that are developed within and characterize their domestic political processes and institutions”; (2) “The anarchic nature of international politics implies that a clash between democratic and nondemocratic norms is dominated by the latter, rather than by the former” (1993: 625). These assumptions jointly construct an explanation based on the role of political norms, which they call “democratic norms,” norms of peaceful resolution of political conflicts and compromise; norms that result in an atmosphere of “live and let live” and a sense of stability at the personal, communal, and national levels (1993: 625). Crucially, these norms are public and are transmitted internationally between democracies (1993: 625) and help to settle their conflicts peacefully through compromise.

The structural theory of democratic peace also is based on two assumptions: (1) “International challenges require political leaders to mobilize domestic support to their policies. Such support must be mobilized from those groups that provide the leadership the kind of legitimacy that is required for international action” (1993: 626); (2) “Shortcuts to political mobilization of relevant political support can be accomplished only in situations that can be appropriately described as emergencies” (1993: 626). These assumptions construct an explanation that stresses the role played by the democratic institutions of checks and balances between the different branches of democratic government, and no less important, between the executive branch and the electorate. These factors place hurdles before a government’s ability to muster legitimacy for unnecessary international adventurism. Additionally, the democratic structure slows decision-making and thus allows time to settle conflicts peacefully (1993: 626).

Although the normative and structural models are presented as different, Maoz and Russett readily admit that they are not mutually exclusive. They only emphasize “different facets of democratic politics that are presumably responsible for the democratic-peace phenomenon” (1993: 626). The question that follows is, then, why bother pinning down one most important cause? Maoz and Russett seem driven by a quest for a parsimonious explanation, to identify that single variable that offers the strongest causal relations. This scientific compulsion drives them to discover that magic ingredient in democracy that produces peace, even when that factor does not, and cannot, exclude other relevant aspects in democracy. Maoz and Russett admit the conundrum, but struggle against it: “Note that our stability measure is not fully distinct from structures. It can also be an institutional constraint in the limited sense than an unstable democracy is subject to overthrow, releasing the institutional constraints on leaders” (1993:

630). Add to that the operationalization of distinguishing the two models: “Obviously, it is extremely difficult to distinguish between these models in terms of contradictory predictions. Normative and structural explanations are often not well differentiated conceptually, thus enhancing the difficulties of testing them as alternative hypotheses” (1993: 626). We are left staggered by the enormous effort to distinguish the two models from each other. Even more, we are puzzled by the conclusiveness with which Maoz and Russett decide between them. For Maoz and Russett, although the structural model provides some explanatory power, the normative model carries the day and is hailed as the valid theoretical explanation (1993: 634–635).

This certainty is especially mystifying once we note two other features of the analysis in Maoz and Russett (1993). The first is that although they try to quantify all the data, they admit having to apply their judgment to the most problematic – and critical – cases (1993: 628). A second problem is that due to the difficulty of measuring norms they use mainly structural criteria to measure the degree of democraticness of different states: “the level of authority of a political system as a combination of (1) competitiveness of political participation, (2) regulation of participation, (3) competitiveness of executive recruitment, (4) openness of executive recruitment, and (5) constraints on the chief executive” (1993: 628). These are structural criteria. And to measure democratic norms they use the highly problematic proxy measures of deaths from political violence and extent of domestic conflict (1993: 630). These proxies are based on the plausible assumption that democratic norms prevent violence and death. But this is precisely what they ought to have established and proved.

The article therefore tells us that Maoz and Russett fell into the trap of assuming what must be proven, which leaves us with a discrepancy. Maoz and Russett declare they have scientifically and quantitatively examined the two models, the normative and the structural, but the measurements they actually employ are structural, and based on personal judgment. It is therefore surprising to find these researchers convinced of the superiority of the normative model, and it is important to challenge their assurance. Their conclusion is certainly not based on positivist proofs. On the contrary, positivist methodology has become so knotted that there is only one way out – a priori normative assumptions. The theoretical conclusiveness enjoyed by Maoz and Russett in fact stems from their liberal moral convictions and commitment to the normative, participatory, and deliberative understanding of democracy. We also find support for this in other writings by Russett where he shows a clear commitment to the normative and participatory understanding of democracy (see, for example, Alker Jr. and Russett, 1965; Hartley and Russett, 1992; Maoz and Russett, 1992; Russett, 1993b; Russett *et al.*, 1996).

A final important point with regard to Maoz and Russett’s article is that they conclude with a normative assertion:

But if enough states become stably democratic – as may be happening in the 1990s – then the possibility emerges of reconstructing the norms and rules

of the international system to reflect those of democracies. A system created by autocracies may be recreated by a critical mass of democratic states.

(1993: 637)

This is a normative conclusion that goes against Maoz and Russett's positivist commitment that assumes and prescribes a sharp distinction between the research and what is being researched, as well as between fact and value. To blur these two distinctions undermines positivist aspirations toward science. Maoz and Russett's factual analysis is entangled with normative goals. All of this calls back to Jackson's critique of neopositivist theory in Chapter 1; the approach to research is not fully objective, at least in the preceding example, and entails a largely implicit philosophical viewpoint.

A second article exemplifying normative theory is Dixon's article "Democracy and the Peaceful Settlement of International Conflict" (1994). This study is infected by the same bewilderments reflected in Maoz and Russett's work. Dixon examines the two explanations for democratic peace, opting for the normative model to explain why democracies settle conflict peacefully. Dixon emphasizes broad consensus over the fact of democratic peace, yet no agreement about its explanation (1994: 15). Like Maoz and Russett, he decides between the two models, even though he admits, "norms and institutions go hand in hand and any effort to untangle their causal implications faces serious obstacles" (1994: 15). To confront this, Dixon says, we must begin "with a clear conception of democracy" (1994: 15). And the essential feature of democracy is for Dixon the norm of bounded competition (1994: 15). Moreover, in an act that links him to the normative and participatory understanding of democracy, Dixon assigns extensive roles to citizens:

Competitive processes may take a wide variety of forms and involve many types of actors, including the political elites who legislate and execute policy at all levels and the citizens who organize and express their interests through a bewildering array of associative arrangements.

(1994: 15)

All of that points to Dixon's endorsement of the normative understanding of democracy. When he tries to measure the indicators of democracy, the picture becomes clearer still. While he supposedly examines the normative model and embraces a normative conceptualization of democracy, he brings as corroboration those same structural indicators endorsed by Maoz and Russett. He explicitly identifies them as institutional indicators (1994: 21). Structural indicators are the easiest markers, and maybe even the only operationalizable method, to measure democracy and identify a parsimonious explanation (1994: 23). But then you wonder if this involves looking for a coin under a street lamp when you know you lost it somewhere else, somewhere dark and unlit by street lamps. Dixon does not engage himself in such questioning. He sets himself the awkward task of proving the superiority of the normative model by examining structural

criteria. It is the same problem identified in Maoz and Russett, and I offer the same answer as I offered above. We can understand this puzzle if we understand Dixon's moral convictions, and his commitments and allegiance to the normative and participatory understanding of democracy – commitments that are also evident from the political roles he assigns to democracies' citizens. Once again the analysis from Jackson in Chapter 1 comes to mind: neopositivism claims the high ground of science but cannot and does not operate in the fully detached manner that its advocates might claim.

The question is: Can we find the same results in the work advancing structural explanations? Structural theory analyzes how the structure of democracy generates mechanisms that reduce the chances of war between democracies. I focus on two exemplary articles that demonstrate the sophistication of this theory, but demonstrate, again, that theory is a form of political thought that endows its concepts with meanings borrowed from normative reasoning. The first article is James Fearon's "Domestic Political Audiences and the Escalation of International Disputes" (1994). The second is "An Institutional Explanation of the Democratic Peace" (1999) by Bruce Bueno de Mesquita *et al.*

Fearon's article incorporates the concept of audience cost. Using formal modeling, Fearon explores the impact of audience cost on the dynamics of crisis escalation and de-escalation. Fearon makes strong assumptions that crisis is a dynamic process that builds up in stages. At each stage the parties signal their intentions and their determination. The result is a "war of nerves." Crises are public events in which domestic audiences observe and assess their leadership's performance (1994: 577). Leaders on both sides attempt to assess their rival's intentions, capabilities, and determination and decide whether to take the crisis to the next step, or de-escalate. Fearon additionally assumes that political structures generate different levels of sensitivity to audience cost, which is the price a leader will pay to keep leadership. In a democracy, the audience cost is higher, and a leader who chooses to de-escalate a crisis will find it harder to keep his or her post. Thus, audience cost is a crucial consideration for leaders when deciding whether to escalate or de-escalate the crisis. Moreover, the public in both countries knows the audience cost, as it is communicated publicly between parties to the crisis (1994: 577).

The result is that democracies mean business in their crisis management. No sane democratic leader would choose escalation if she has reason to believe she will have to "chicken out." If a democratic leader opts to escalate, the reasonable conclusion is that she means business and will be willing to go all the way. As in any formal modeling, Fearon's assumptions are strong, even crude, which he readily admits, especially with regard to democratic leaders being more sensitive to audience costs. Fearon acknowledges that "the price of losing power is often greater for a dictator than for an elected leader" (1994: 582). This complicates the matter, and admitting this weakness shakes the reliability of the model.

Fearon shows an entirely structural understanding of democracy. His is a political structure with such a high audience cost that any leader with the desire to keep her post will factor in the consequences of backing down. Democracy is

indeed the kind of regime that due to its *structural* features generates high sensitivity to audience costs. Here, the key concept is formal accountability, conceptualized as the scrutiny of leaders by their citizens ("the audience").

Fearon's understanding of the chances of democracies going to war is ambivalent. He does not rule out the possibility of war. Rather, he couches his belief in the reduced possibility of war between democracies in modest phrasing: "If democracies are better able to communicate their intentions and to make international commitments, then the security dilemma may be somewhat moderated between them" (1994: 587). Realism rules, though in a regulated format, one that accepts the possibility for a rational solution to the allegedly insolvable problem of the security dilemma, though only between democracies.

The same themes are developed further by Bueno de Mesquita *et al.* (1999). The behavioralist perspective of Bueno de Mesquita *et al.* treats norms and values as nothing more than behavioral incentives, where the only universal incentive they acknowledge is the desire to retain office. Like Fearon, they advance relatively simple assumptions. This is especially true with regard to their main assumption that the primary goal of all leaders is to retain their seat. Bueno de Mesquita *et al.* briefly admit the heavier price a dictator may pay in losing office, yet brush it aside (1999: 797). Disregarding any questions as to the verity of this assumption, questions to which they themselves refer, they proceed with their sophisticated theoretical construction. Instead of talking about audience cost, they talk about selectorates and winning coalitions. The former concept refers to all those with a right to participate in choosing a government, the latter is a subset of the selectorate whose support is necessary to form and keep the government (1999: 793).

According to this highly rationalist reading of politics, incumbents do all in their power (and the need for legitimacy is not considered a limitation to executive power) to keep their job. They will tap into scarce national resources in whatever way will efficiently allow them to buy support from the winning coalition. The incumbent must decide whether to use the national resources as a public good to be distributed among all members of the selectorate, or as a private good allocated to the winning coalition. The decision will not be based on norms and values, only on tactical utility. However, the problem in a democracy is that the selectorate is large and so is the winning coalition. Therefore, because of the considerable size of the winning coalition, allocating private goods to them would not do, as each member of the winning coalition would be left with only a small quantity of these goods (1999: 797). Thus driven by necessity, the incumbent will opt for the public good allocation tactic. That means she will be concerned with policy failure, especially with regard to winning and losing wars.

Take note, the conceptualization of accountability employed by Bueno de Mesquita *et al.* is a very restricted and structured one, constrained by the incumbents' calculations of the efficiency of tactics available for buying the job. All is permissible in this view of democracy, and the only difference between democracy and autocracy is the size of the winning coalition as a proportion of the

selectorate. As they emphasize: “Our model explains these diverse phenomena without attributing superior motives or greater civic mindedness to one kind of leader over another. The explanation is driven purely by self-interested leaders who seek to retain office and face alternative institutional arrangements” (1999: 805). This is very much a Schumpeterian perception in which democracy is no more than a competition between members of the elite over governance. The public’s only role is to participate in elections once every few years and decide which contestant wins.

The next theoretical move is that due to their inability to buy their seats with private goods and their consequential need to succeed in their policies, especially their wars, democratic incumbents do not attack if they do not anticipate victory, and they also try harder to win the wars they do fight (1999: 799). And hence, “[b]ecause autocrats do not try as hard in war, they make attractive targets for democracies” (1999: 799), and democracies will tend not to attack each other. But as this is the reason and not some sort of normative commitment or democratic communal bonding, the peace between democracies is indeed merely the probabilistic and temporal absence of war. There is no guarantee for peace, and when two democratic leaders are afraid of unsuccessful domestic policies it is quite likely that they will opt for diversionary war between themselves (1999: 804).

Thus, the way we conceptualize democracy helps to fashion the way we conceptualize war and vice versa. The concepts endow each other with meaning. Likewise, through their essential contestedness, concepts, as the building blocks of theories, embed the normative within the explanatory. Definitions of essential contested concepts are not, and cannot be, objective and neutral. Definitions are heuristic devices that help in delineating empirical phenomena and categories from each other, and the delineation, as we have seen, is morally based no less than empirically founded. Hence, definitions are the interpretative meeting points of empirical observation and moral commitment. Understanding the normativity within the explanatory forces, the philosophy of the social sciences (IR included) should focus not only on questions of epistemology, but also on ethics. Philosophy of the social sciences cannot satisfy itself with improving the explanatory power of theories but must also deal with enhancing the moral reasoning of theorists. This is especially true because the two concerns, the epistemic and ethic, are mutually constitutive. *Zooming In Zooming Out* addresses this obligation.

Zooming In Zooming Out

Emphasizing the importance of concepts in theoretical work and their contestedness brings to the fore the epistemological and methodological strategy of *Zooming In Zooming Out*. It is a double-faceted strategy where we zoom into the internal components of our theories, namely the concepts, and at the same time conceptualize them normatively and with moral sensitivity while taking note of their effects outside academia.

The first action, which focuses on the definition of concepts (*Zooming In*), is not novel taken alone. We are expected to take our concepts seriously and we

receive methodological training to define them. Definitions should provide us with as precise as possible a description of the relevant concepts; descriptions that enable us to clearly identify any social object which falls within the purview of our study, and filter out those that do not. A definition must be exhaustive in the sense of including all social objects captured by the concept and exclusive in the sense of ruling out all social objects outside the concept's domain. Another definitional criterion is operationalization. This means that the definition will be helpful in making the theories and/or hypotheses testable and refutable and instrumental in measuring the phenomenon being investigated. Together, exhaustiveness, exclusiveness, and operationalization provide us, or so the conventional wisdom goes, with a scientific apparatus with which to develop and test hypotheses. Moreover, as definitions are supposedly transparent and neutral, they can support a rational and objective concurrence among the theorists; an agreement that is free of any moral commitments and political convictions.

But if this depiction of defining our concepts is true, why are those very concepts contested? For it is that contestedness that plagues our studies and, as we saw in the previous section, embeds explanatory theories within moral commitments – from which they supposedly are free. Answering this puzzling question marks the first stage in justifying *Zooming In Zooming Out*. In the social world, definition is the bounding and rounding of that which is unbounded and unrounded. In the social world, things do not fall that neatly into human-made concepts. Boundaries are fuzzy at best. Social objects are related to each other by various comparable features and separated by other distinct features. Hence definition is about establishing an essentially arbitrary delineation between social objects, phenomena, or processes. It inflates the importance of certain differences in order to separate certain phenomena and processes (rounding), and undermines and even ignores the relevance of other differences to combine other phenomena and processes (bounding). This is the only way to arrive at definitions in the social world, and therefore definition involves the social construction of social categories.

Think of peace and democracy. The figure of 1,000 deaths is an arbitrary boundary³ that differentiates between conflicts where there are 1,000 dead and which are therefore identified as wars and conflicts where *only* 999 are killed (as if one death can make all the difference between war and no-war). And a Polity II score of six serves the same purpose, of arbitrarily differentiating democracies from non-democracies (see, for example, Dixon, 1994). There is nothing exogenously obvious about these conceptual (and measurable) boundaries, a conclusion that supports the argument regarding the contestedness of concepts. Boundaries between concepts and categories are not objective; they are embedded within moral commitments. Definition is a moral and political act, even though not necessarily consciously so. Hence the normative content of those concepts which are taken as neutral; hence the contestedness of that which was supposed to act as the foundation for rational and objective concurrence and scientific certainty.

However, concepts' contestedness and their moral contents are neither perceptible to the public eye nor largely to the scholarly eye and instead are hidden

by the definition's operationalization. Operationalization entrusts research with the language of objectivity and works to hide the moral commitments of the concepts that form the building blocks of explanatory theories – moral commitments that are at work whenever a researcher defines the concepts she is working with (e.g., democracy and peace), when she theoretically constructs the social categories she will later gear into a rigorous research program. Hence, operationalization obfuscates theories' normative underpinnings. Moreover, it encourages theorists to seek measurable variables even when these variables do not exactly conform to their theoretical framework. This dynamic was especially evident in the case of Maoz and Russett, who withdrew to the more measurable proxy criterion of political violence when attempting to prove the power of democratic norms, and in the case of Dixon, who used structural indicators for testing his normative theory.

These observations lead to the second and more contested dimension of my strategy: *Zooming Out*. In addition to the obfuscation of the normative dimensions of theory, also theorists' moral, political, and social responsibilities demand *Zooming Out*. The concepts we use in our explanatory theories are contested, they have several possible meanings, at least some of which are legitimate, and each of which is informed and reasoned by a different moral framework. Notwithstanding the operationalization of the concepts and the language of scientific objectivity used by theorists, to choose one meaning is to participate in the political arena and embrace one moral framework. Furthermore, sometimes this choice has real-world ramifications if explanatory theories migrate outside academia. The democratic peace theories provide ample evidence of this migration and the interweaving of academia and politics (Ish-Shalom, 2013).

For the above reasons, *Zooming Out* accompanies and supplements *Zooming In*. *Zooming In* asks us to focus our theoretical rigor on better defining the concepts we use. *Zooming Out* rejects the criteria of exhaustiveness, exclusiveness, and operationalization as inadequate when standing alone. *Zooming Out* burdens us with the obligation of defining our concepts morally. It asks us to be ready to justify morally the definitions with which we operationalize the concepts we use when theorizing. To be able to do that we must also reflexively and critically engage with our own moral commitments which inform (sometimes unconsciously) our theoretical definitions. It also calls on us to morally judge the possible real-world ramifications of our theories. Not all possible ramifications are foreseeable, but we have to try our reasonable utmost to anticipate real-world ramifications and evaluate them morally.

Though at first glance this strategy appears modest, it carries with it fundamental implications for social research. One obvious outcome of the *Zooming In* *Zooming Out* strategy is improved dialogue between political science and moral and political philosophy. If, to some within political science, this outcome may seem a devastating blow to their scientific integrity, this is not the case with moral and political philosophers. Keen and perceptive philosophers are usually attuned to the realities of the world and to the theories that try to explain them

(see also Chernoff, 2009b: 161; Enoch, 2002: 240–243; Taylor, 1985: 65–66). This is obviously the case with consequentialism, which addresses the outcomes of actions (or rules) and requires the ability to somehow forecast those outcomes with the help of causal mechanisms linking acts (or rules) and outcomes. Those causal mechanisms are sought in political science theories (see Singer, 1972: 241).

The same interest in examining real-world processes through social and political theories is found in deontological moral theories. Deontologists, too, rely on causal mechanisms borrowed from social and political science. C.A.J. Coady offers this deontological argument: “Even those of us who think that truth, in some substantial sense, does apply to moral discourse need to acknowledge that moral truths are supported by practical reason and are dependent in complex ways on issues of practicality” (2004: 788). For example, we find this deontological reliance on explanatory theories in just war theory, which argues: “if there is no probability of achieving the just causes, the war’s destructiveness will be to no purpose” (Hurka, 2005: 35). The ability to predict those probabilities does require reliance and familiarity with the relevant political science theories.

But we need not stray far from the democratic peace thesis for an example of political philosophers relying on political science theories. A good example is John Rawls, who gave the theory of democratic peace as evidence of the practicality of his *Law of Peoples* (1999). He worked hard to try to persuade readers that his proposal was a realistic utopia, something to which we could reasonably aspire, albeit with great effort and pains. Rawls acknowledges that he has to “offer a reply to political realism as a theory of international politics, and to those who say that the idea of a realistic utopia among people is quixotic. I do so by sketching a view of democratic peace” (1999: 44). It is evident that Rawls the philosopher was aware that he also needed to communicate with political science theorists, that he needed to address their concerns. He did this by basing his arguments on the theories of democratic peace (1999: 44).

Note that Rawls also accommodated the democratic peace to his own political philosophy, and in doing so reformulated the theories, making them more deliberative. Below we will examine his philosophical move. For now it is sufficient to note that philosophers like Rawls are aware of the work taking place in political science. Moral and political philosophers perceptively rely on causal mechanisms to establish their justificatory schemes. In other words, they keep social science theories in mind when setting up their political theories. *Zooming In Zooming Out* calls for social and political scientists to supplement the philosophers’ awareness and reliance on them with a complementary awareness and reliance of their own. To do this, they must be aware of the moral and analytical work moral and political philosophers are undertaking. That is not to suggest they must invent the moral wheel. They have to be able, self-reflexively, to use moral definitions to back their moral commitments. Note, as discussed above, that theorists’ moral commitments are operative anyway when defining concepts and during subsequent theorizing. Usually, though, (mostly positivist) theorists

are unaware of any inherent moral aspects involved in defining and theorizing, and hence fail to respond to them appropriately. Furthermore, operating in the darkness of unawareness, moral commitments are not fully developed, and at times cause internal weaknesses, clashing as they do with the operationalization-related requirements of research. The previous section offered ample examples of such internal weaknesses, as in the study by Maoz and Russett and in Dixon's treatment that substantially conceptualized democracy as normative and deliberative, yet operationalized it structurally and procedurally.

Accordingly, *Zooming Out* obligates a more conscious, reflective, and responsive attitude toward the workings and implications of moral commitment. It calls for moral commitments and their implications to be explicit and transparent to both theorists and those they theorize about in order to enable theorists to justify morally the definitions they chose when theorizing and to engage critically with their own normative commitments. Such engagement can achieve a deeper level of self-reflection and self-criticism and contribute to improving the definitions of concepts theorists employ both in moral and analytical terms. The result will improve our theories. I will not pretend this is easy; it is daunting. But it is doable, and our scholarly and social responsibilities charge us with this normative mission. In one way it parallels the call from Fred Chernoff in Chapter 5, within the context of assessing theories, for explicit statement of criteria for evaluation. Progress is impeded, in a word, by ambiguity.

Again, Rawls offers an example of how engagement works at the philosophical end of the bridging movement in the *Zooming In Zooming Out* strategy. Rawls indicates that the peace resulting from the democratic peace is not a temporary balance of forces. Rather it is peace which is gradually stabilized for the right reasons, "a situation in which, over the course of time, citizens acquire a sense of justice that inclines them not only to accept but to act upon the principles of justice" (1999: 45). Rawls's understanding of peace is of a stable and positive peace. Furthermore, it is not interests defined as the political survival of this or that elite that are responsible for achieving a (temporary) peace, as explained by the structural theories of democratic peace. It is justice and its principles functioning as a moral public compass, and achieving this through citizens who internalize and act upon them. Civil society is the anchor of peace, not the political system; citizens and their values establish and guarantee democratic peace, not politicians and their interests.

This definition and conceptualization of democracy is inclined toward the normative end of the scale, and Rawls is explicit in embracing the deliberative model of democracy. According to Rawls, contemporary existing democracies may indeed appear as formal constitutional regimes. However, these are not the democracies that can produce the ideal of democratic peace – stable and positive peace brought about by the right reasons. The democratic ideal which will bring about democratic peace is one that guarantees the public capacity for deliberation. It is a democratic model that enables the production and elucidation of public reason and hence the translation of citizens' preferences into public policies and public goods. To achieve this, a democracy must meet five criteria:

(a) A certain fair equality of opportunity, especially in education and training.... (b) A decent distribution of income and wealth.... (c) Society as employer of last resort through general or local government, or other social and economic policies.... (d) Basic health care assured for all citizens (e) Public financing of elections and ways of assuring the availability of public information on matters of policy.

(1999: 50)

Rawls is adamant that this definition and conceptualization of democracy is not compatible with libertarianism. He sees democracy going hand in hand with liberalism, liberalism as the proponent of social welfare principles and a basic structure which allows citizens to take part in public deliberation and promote public reason along with the public policies and the public goods that dovetail in. Only thus can we approach the ideal of democracy and a real and stable democratic peace.

What is evident in Rawls's discussion is that the explanatory dimension is secondary. He aims for a normative analysis, and it is this normative aim which drives and directs his discussion. The democratic peace theory merely provides an empirically plausible, causally valid foundation on which to build his normative theory. Rawls treats the explanatory dimension of his work functionally, and as such it is quite embryonic. *Zooming In Zooming Out* calls for a comparable move by social science theorists where they use moral and political philosophy as a morally justifiable and permissible foundation for building an explanatory theory. And as their main aim and task is explanatory, their treatment of the normative dimension can also be functional and embryonic. According to the *Zooming In Zooming Out* strategy, this will suffice, though it is also of fundamental importance.

As a final note, I will draw a preliminary chart of how we can define and theorize in accordance with *Zooming In Zooming Out*. I do this by taking the democratic peace as the example, though it should be clear that *Zooming In Zooming Out* should be employed by all social science theorists working with any type of social science theory. First, in this example, we must critically base ourselves on a normative theory of democracy. Problematic though this may be, my moral commitments are linked to the deliberative and participatory models of democracy. These are the models that duly respect our humanity, sociability, the reasonableness of our judgment, and our political faculties. As de-Shalit forcefully argues, "we value participation itself, not simply as a means of reaching decisions" (1997: 74). Taken together, participation and deliberation are the political apparatus that give due respect to the above human traits and faculties and help to actualize them. This is the source for the moral and political superiority of these models of democracy over the elitist and structural models.

The same applies to the concept of peace. We should re-examine how we define peace and decide if we are morally satisfied with a minimal, negative definition of peace, namely peace as the absence of war (Waltz, 1959: 1). Alternatively, theorists can follow Galtung (1969) and Boulding (1979) and define

peace more positively, as a stable and positive state brought about by the right reasons: an established social state of affairs in which war is not an option. Accordingly, they should only consider this stable and positive peace as their dependent variable and as the social category worthy of being defined as “peace,” and of being studied and theorized as such.

Following this initial phase, developed explicitly in our theoretical writing, we must re-examine our operationalized definitions and see if they are compatible with our moral commitments. Defining is a social construction of social categories and as such is an intellectual and theoretical activity with serious moral implications and political outcomes. Reflexivity is therefore necessary when defining and/or applying operationalized definitions; we should take seriously the contestedness of the concepts that comprise our explanatory theories and be willing to scrutinize the morality of the operationalization of the definitions we employ. When there is no correspondence between our moral commitments and the operationalized definition we should redefine the latter to correspond to the former. Moral commitments take precedence over operationalization and hence should govern the act of defining and theorizing.

Once this normative phase is over, our task as social scientists is to develop the explanatory mechanism of the democratic peace – a morally and empirically grounded democratic peace theory. As advised here, this should center on deliberation and participation as both a moral and explanatory core: how deliberation and participation are morally justified (being the best actualization of our humanity, sociability, reasonable judgment, and political faculties) and causally implicated in bringing about stability for the right reasons, that is, true and real democratic peace.⁴

Conclusions

The essential contestedness of concepts such as peace, war, and democracy defies positivism and objectivity. Theories are built of essential contested concepts and thus are value-laden. The normative groundwork of theories is evident in the democratic peace literature, which is divided along a moral axis into two families of theories. The first, normative, is more liberal and founded on conceptualizing democracy as a participatory and deliberative sociopolitical project and on peace as a comprehensive phenomenon; the second family is more conservative and founded on a structural understanding of democracy as a regime founded on contest between political elites over ruling the polity, and on peace as the transient absence of war. It is this fundamental normative difference rather than mere differences in the explanatory mechanism that divides the different theories.

Hence, to achieve progress, there is the need of a moral philosophy of the social sciences, combining epistemology and ethics to promote better theorizing; better both in terms of knowledge production and in terms of normative reasoning. Theorists should assume and discharge moral responsibility for their theories. The way to discharge this responsibility is by adopting *Zooming In*

Zooming Out. This strategy calls upon theorists to zoom into the internal components of their theories, namely the concepts which are essentially contested, and define essential contested concepts with moral sensitivity. *Zooming Out* adds normativity to the commonly accepted criteria of exhaustiveness, exclusiveness, and operationalization. It asks theorists to be ready to morally justify the definitions with which they operationalize the concepts they use, and to do this theorists must critically engage with the moral commitments which inform their work. These changes offer a way to operationalize sociable pluralism, the present volume's central theme as articulated by Freyberg-Inan in Chapter 4, through recognition of the need for dialogue between and among empirical researchers that includes a place for connection of their studies to the well-being of people in general.

Notes

- 1 Following Sandel's (1984) critique of Rawls (1971).
- 2 I embrace the view that deliberative and participatory models of democracy are complementary. Mutz (2006) advances an opposing and interesting position that sees the two as contrary.
- 3 The exact definition given in the Correlates of War Project is: "an interstate war must have: (a) sustained combat, involving (b) regular armed forces on both sides and (c) 1,000 battle fatalities among all of the system members involved" (Sarkees and Schafer, 2000: 125).
- 4 Elsewhere (2013), I argued for the need of theorists to take upon themselves the role of theoretician-citizens. This role is another normative response (besides *Zooming In* *Zooming Out*) to the set of responsibilities academia bears to society.

9 **The closer you look, the less you see**

Knowledge cumulation in IR

Laura Sjoberg

So often in IR we ask questions about what we have learned – what is the original contribution of this research? How could you tell? Did this project use reliable methods to provide evidence for this hypothesis? Does this research program have potential for knowledge accumulation? What do we do this for? Will this be worthwhile if we come close to science? If we can call IR science, can we call what we do knowledge? Or progress? I argue that these questions are at least partial representations and at worst, quite simply, the wrong questions. When it comes to standards for knowledge cumulation in IR, the closer that you look at those standards' foundation and substance, the less stable they appear.

This chapter explores what is made invisible in traditional understandings of knowledge in political science, the disciplinary politics of knowledge construction, and the exclusionary impacts of knowledge-policing standards. Given these problems, there is advantage in valorizing failure in knowledge production, and understanding knowledge cumulation in IR as productive fantasy. The chapter concludes by contending that the future of the discipline is in the manifestation of the productive fantasy of knowledge cumulation in argumentation. The chapter uses the democratic peace research program as an example of the ways in which the fantasy of knowledge production is produced and reified, and to envision possible alternative futures for IR.

What IR does not see

It is as important to see what we *do not* learn from IR analysis as traditionally understood as it is to see what we *do* learn (perhaps even more important). Our silences tell us more about the state of knowledge accumulation in the discipline than looking for standards that tell us what we do know. This inspiration comes from a long tradition of feminist research methodology that emphasizes how important it is to search for where women are omitted, excluded, kept out, and not mentioned in order to understand how women are constituted, where they are, and what happens to them in global politics (Keller, 1985; Scheman, 1993; Tickner, 1988; Charlesworth, 1999; Kronsell, 2006; Peterson and True, 1998). This work in feminist International Relations has directed attention not only to operational and actual silences but also to things generally made invisible in

traditional analyses of global politics – the margins, the subaltern (Peterson, 1992; Tickner, 1992; Parpart, 1993; Nayak and Suchland, 2006). I apply this methodological analysis to methods of thinking about how the discipline works – to reveal that what IR does not talk about is as important as the substance of current IR research, *both* substantively and epistemologically.

For example, I work on intentional civilian victimization (Downes, 2006; Valentino, 2004; Kalyvas, 2008; Sjoberg and Peet, 2011). That literature is concerned with explaining wartime strategies that kill (or attempt to kill) noncombatants in interstate and civil wars. Regime-type, belligerent desperation, power difference, and cultural difference are discussed as potential explanatory factors. Intentional civilian victimization is defined/operationalized as either direct killing (execution, gassing, or bombing) or indirect killing (starvation, exposure, and disease by blockade, relocation, or forced labor) (Downes, 2006: 10). Feminist scholars have provided ample evidence of the prevalence of a type of intentional civilian victimization: conflict sexual violence (Card, 1996; Kirby, 2012; Jeffreys, 2007; MacKinnon, 1994; Niarchos, 1995; Peterson, 1977; Grossman, 1995; Albanese, 2001; Anonymous, 2005; Seiffert, 1994). Conflict sexual violence, however, is not included in operationalizations of intentional civilian victimization. When I asked a scholar in that research program why not, s/he suggested that it was because “intentional civilian victimization” is “limited to killing,” and rape, while terrible and tragic, is not killing. Of course, though, the feminist literature shows rape to be about killing, extermination, and even extinction in war and conflict (e.g., the discussion of war rape as genocide – see MacKinnon, 1994; Sharlach, 2000; Hansen, 2001). In my view, the silence of the intentional civilian victimization literature (and most research on strategy) on gender issues generally and on conflict sexual violence is constitutive of that literature – a condition of possibility of its definitions, its explanations, and its parameters. The literature is set up in a way that makes my desired feminist intervention impossible, by definition.

With feminist scholars before me, I am interested in constitutive silences that create conditions of possibility for existing research programs. For the democratic peace, that means figuring out what the literature does not address – substantively, methodologically, and personally. While I do not consider myself an expert in the democratic peace literature or a member of the research program, I have engaged the work (Sjoberg, 2013; Sjoberg and Knudson, forthcoming). Existing critics of the democratic peace research program point out that it neglects: collective security (Cederman, 2001b), economic liberalism’s key role (Cederman and Rao, 2001),¹ common interests among democracies (Gartzke, 2000), U.S. exceptionalism (Rosato, 2003), territorial settledness (Gibler, 2007), selectorate effects (Bueno de Mesquita *et al.*, 2005), and gender-based dynamics (Caprioli, 2000). Methodologically, critics worry about the theoretical significance of anomaly cases (Chan, 1997; Spiro, 1994), the tendency to minimize negative cases (e.g., Owen, 1997; Tremblay and Schofield, 2005), and problematic operationalizations of democracy, war, and peace (Chan, 1997).

These critiques are part of what it means to search for what is unsaid in a research program – what variables does the research program not take account of? How could accounting for those variables change the analysis? I am arguing that there are *visible* omissions (like the variables that the democratic peace research program's critics urge incorporating), and *invisible omissions*. Invisible omissions are those that are unheard by a research program – normally left out or ignored by *both* the researchers that form the core of the research program *and* their critics. These are like the omission of conflict sexual violence from the intentional civilian victimization literature. Unlike its visible omissions – variables that its scholars and their critics have added to, re-operationalized, expanded on, or suggested the inclusion of – conflict sexual violence is unheard within that literature.

In my reading of the democratic peace literature, the unheard is located in the assumed normative good of the term “democracy” and the states that are classified as democracies. Scholars have studied the violence that goes into democratization, as a trend (Mansfield and Snyder, 1995b) and in specific instances (e.g., the United States and Native Americans in Strickland, 1986; the United States and slavery in Fehrenbacher, 2001; New Zealand and Maori people in MacDonald, 2003). Democracies have been built by killing people who did not fit within a particular political community or idea of government. Frequently, states are classified as democracies when they enslave parts of their populations *legally* (e.g., the United States 1815–1863); when they have excluded a majority of their population from participating in electoral processes (e.g., the United States 1815–1920; Great Britain 1815–1928; France 1848–1852, 1870–1940); and when they have engaged both colonialism *and* wars to fight against colonial independence (e.g., nineteenth- and twentieth-century France, UK, Netherlands, United States, Belgium, France, to name a few). Defenses of the classification of these states as democracies often reference an evolution of the concept of democracy which has *come to* include women and minorities though it did not once. Those defenses legitimate the exclusion that those “democracies” engaged in “back then.” States *currently* classified as democracies have violence *within* their borders that often disproportionately impacts their people's freedom and rights but is not accounted for in defining democracy: from organized crime (Alum and Siebert, 2004) to sexual violence (Campbell, 2014), from inhumane working conditions (Johnson, 2014; Pennock, 2014) to extreme poverty (Hickey and Bracking, 2005). These disenfranchise people politically, even when they are not disenfranchised by law (Weaver *et al.*, 2014; Ma, 2014; Kwagha, 2013). Another key silence of the democratic peace research program is democracies' conduct in the international arena. Many of the early democratic peace theorists suggested that democracies are *as likely to make war as other states* (Russett, 1993b), but unlikely to make wars against democracies. This statement indicates the possibility that democracies are *more likely* to engage in conflict with non-democracies than the average country is to make war. These wars can be wars of democratization, wars of imperialism, wars of resource exploitation, or wars for other reasons – but democracies often

start them (Mansfield and Snyder, 1995b; Hardt and Negri, 2005; Schweller, 1992; Barkawi and Laffey, 1999). Are violent histories, exclusive standards, silenced marginality, and bad behavior *outside* the community of democracies conditions of possibility of democracy and therefore of the democratic peace? Questions like this are the unhearable omissions of the democratic peace research program – unhearable by both its proponents and its opponents.

A politics of knowledge

Leveraging this analysis of silence, I explore the argument that the discipline's standards for knowledge production are political and performative rather than given, objective, useful, or founded. I mean performativity in the sense that Judith Butler uses it (Butler, 1990, 1993), particularly as she talks about it going hand-in-hand with a Foucauldian notion of disciplining (Foucault, 2003; Edkins, 1999; Steans, 2003),² where "performativity cannot be understood outside of a process of iterability – a regularized and constrained repetition of norms" which resonate as "ritualized production" (Butler, 1993: 60). This frames performativity as a "specific modality of power as discourse" (1993: 139), where the politics of the signification and the politics of the sign meet, an act of territorialization, of production, of installation – which does not have to be alone, singular, or unidirectional. Since performatives are their own referent (1993: 159), they proliferate as manifestations of the power underlying them, and interact relatively on the basis of that relative power. In this way, "the reduction of performativity to performance would be a mistake" (1993: 178). It is both the utterance and the power structures that constitute performativity, where the utterance is sufficient for performance. As such, whoever the performer is, "the reach of their signifiability cannot be controlled by one who utters or writes, since such productions are not owned by the one who utters them" (1993: 185), which creates a by-definition ambivalent implication and sense of power.

It is my suggestion that what counts as knowledge in the field, in particular research programs and more generally, is performative – where standards are set by their utterance and repetition rather than by some external "objective" standards of good science (narrowly) or good research (more broadly). The iterative performance of standards of the measurement of knowledge has the impact of rendering uninhabitable the methodological, epistemological, and political space that falls outside of those performed standards – quelling dissent.

Butler provides an account of how that happens. She argues that, in the neo-liberal capitalist imperium (Agathangelou and Ling, 2009), "dissent is quelled ... through threatening the speaking subject with uninhabitable identification" (Butler, 2006), given that a dissenter must choose silence over being labeled treasonous or some other label that has an identification with evil. This catch-22 limits what we can hear, what we can see, and what we can mourn, and also how we live, which she frames (in Levinas's terms; Levinas, 2005) as "the surplus of every sociality over every solitude" (Butler, 2006). Butler argues that it then constitutes the over-infusement and policing of meaning, which serves not only

the performative function of constituting human experience but also the regulatory function of rendering uninhabitable political space (deemed) unacceptable.

This makes statements like “this is good science” and “these results are robust” signs without referents used to discipline (Baudrillard, 1993). For example, I recently edited contributions from six white U.S. IR feminists into a collection titled “The State of Feminist Security Studies.”³ While the intention of the essays was debate about the popularity of sex, not gender, analysis in the U.S. subfield of Security Studies, the combined broad title and narrow contributor list was read as exclusive, “with diversities and differences edited out of the picture” (Shepherd, 2013). A responding forum expressed concern about the disciplining nature of the appearance and intent of the original forum, suggesting it functions as a site of “hegemonic power,” “epistemic violence,” and “censorship,” understood to be “parochial,” “intolerant,” and “imperial” (Shepherd, 2013; Parashar, 2013; Sylvester, 2013; Basu, 2013; Teaiwa and Slatter, 2013; D’Costa and Lee-Koo, 2013; McLeod, 2013).

There are multiple levels and directions of performative disciplining by the imposition of “standards” of scholarship, citation, and engagement here – each of which bears recognition. There is the suggestion in the U.S. IR mainstream that feminist post-positivist research does not have a research agenda (Keohane, 1989; Weber, 1994; Tickner, 1997; Marchand, 1998), and the related celebration of quantitative, positivist, self-identified feminist research in Security Studies (Hudson *et al.*, 2012; Reiter, 2015; McDermott, 2015). The intent of the forum was to expose the disciplining move of privileging over-simple and often incorrect sex analysis over gender analysis – something the forum accomplished. At the same time, the unintended consequence was to discipline-out non-white, non-American feminists who see themselves as engaged in Feminist Security Studies but not in a conversation about its state. The characterization of both the work and myself as racist also served a disciplining function, implying a particular standard for race analysis that scholarship ought to meet that my work did not, and therefore my work did not count as acceptable scholarship.

The politics of exclusion are among the substantive silences discussed above and the impacts of epistemological standards discussed below. For now, the recognition of scholarly accomplishment (and scholarly failure) as performed, reified, disciplining claims rather than objective measurements is important. It is important both on its own merit, and because the claims to simple, “objective” judgments of scholarship make it difficult to see the exclusions produced by those standards and boundaries, whether they are epistemological (like my issue with Security Studies in the U.S.), representational (like my critics’ issue with my work), or questions of licensure to speak (Sjöberg, 2014a). While the substance of the work is constituted by reactions to it, readings of it, and understandings of it, the political economies of signification in disciplinary IR filter those reactions into hearable and unhearable ones.

For the democratic peace, this means questioning the disciplining effect of the work in two ways. First, the democratic peace research program is implicated in

the discussion of the standards of science and methodology (Ray, 2003; Slantchev *et al.*, 2005). Claims about different variables related to and results of hypotheses of democratic peace are justified by recourse to questions of the usage of appropriate methods and the appropriate deployment of those methods (Chernoff, 2004). Second, the democratic peace research program can be discussed as *disciplining*, both within IR and for policymakers who engage with the work. For example, if the presumed normative good of democracy is paired with the presumed normative good of peace, then counter-discourses become difficult to consider. This is all the more true in policy arenas, where policymakers (like President Clinton, as well as other executives before and after him) endorse peace among democracies as a discursive demonstration of commitments both to leadership and to peace (Schafer and Walker, 2006; Gaddis, 2002). The democratic peace, then, has a disciplinary and disciplining life above and beyond any theoretical sense it makes or empirical evidence that its researchers supply for it. It becomes an utterance for utterance's sake, above and beyond any research done to confirm or deny the "truth value" of that utterance. That distances the sign ("democratic peace"/"liberal peace") from any referent (democracy or peace in the world) that it originally may have had. In Baudrillard's term, then, the "democratic peace" as an empty (or even emptying) signifier carries with it both normative and political risks. One of those risks, discussed below, is the discursive framing of exclusion as standards of method and substance.

Excluding by knowing

The invisible disciplining nature of the performative standards of knowledge cumulation is half the story of Butler's understanding of performativity and of my journal forum. The other part is who is excluded by claims to knowledge cumulation, what is left out, and on what axes.

Some accounts of exclusion are straightforward – where early feminists in IR were able to ask "where are the women?" among IR scholars and in IR scholars' readings of global politics (Pettman, 1996; Enloe, 1989; Zalewski, 1998). Similar questions could be asked about the hegemony of U.S. scholars in IR (Hoffman, 1977; Paolini, 1999), the exclusion of minorities *within* U.S. IR (e.g., Turner *et al.*, 2008),⁴ and the intersectionality of exclusions on the basis of race, class, and sex (Chowdhry and Nair, 2002; Agathangelou and Ling, 2004; Mohanty, 2003). These are related to, but do not map directly onto, exclusions based on positivist/post-positivist and qualitative/quantitative divides.⁵ Agathangelou and Ling characterize these forces as interaction in:

An overarching hegemonic project ... it encompasses states, governments, classes, and sets of ideologies that work in tandem to validate each other ... a set of *social relations of power* expressed through daily interactions and the institutions that support them.

(Agathangelou and Ling, 2009: 2)⁶

In Agathangelou and Ling's explanation, the "neoliberal imperium" "draws on and legitimizes neocolonial strategies of power based on race, gender, sexuality, and class to privilege the few at the expense of the many."

This can be seen in my discussion of exclusion in Feminist Security Studies – where a wide variety of different exclusions are structured by claimed standards for the production of legitimate knowledge. It can also be seen in the underrepresentation of scholars from outside America and Western Europe in journals and with publishers.⁷ Some suggest that these exclusions are a result of particular, narrow forms of knowledge accepted by those journals and publishers. In my experience, though, the relationship has been *hybrid*.⁸ For example, American and Western European textbooks often sell all around the world, and many developing PhD programs in countries that previously did not have them use metrics like publications in ISI-ranked journals and internationally recognized book publishers to award promotions. There is not only rejection, but also appropriation, of performed disciplining/disciplinary standards that function to exclude.

These disciplinary standards (both in the traditional and Foucauldian sense) make invisible their own impossibility. For example, a submission to a traditional IR journal in the United States or Western Europe that makes an interesting argument, but is not in the format of, methodologically acceptable to, inclusive of the same forms of evidence traditionally used in, and good science to that journal's traditional reviewers is unlikely to succeed in getting published. This will generally be justified with reference to the "quality" of the piece, and rarely if ever will questions of sex, race, gender, class, and other axes of exclusion be discussed as *producers* of the standards that then exclude on "quality" where "quality" has been set up in a way that excludes all performances of scholarship which are not mimicry of a particular Western, liberal model (Paolini, 1999). Even editors and reviewers who note the exclusionary *effect* of these standards will often mourn that and move on, seeing the alternative of *lacking standards* as more insidious.

The answer to this quagmire is sometimes a liberal politics of inclusion – how do "we" get more women, more minorities, more people from underrepresented places in the world to be *able* to *meet* the standards of good scholarship in the field? That liberal politics of inclusion, while well-intended, can be read as a (subtle, perhaps accidental) expansion of the violence it (formally) seems to abate. As Jasbir Puar argues, the inclusive expansion of the neoliberal imperium to involve the queer other-within remains exclusive and violent toward its constitutive other(s) even as it appears gentler (Puar, 2006). That violence is the violent reproduction of naturalized, bounded identities when identities are liminal and messy when not policed (Haritaworn *et al.*, 2013; Agathangelou, 2013; Scott, 2013).⁹ The bounded nature of IR exclusion excludes liminality, messiness, and outside-ness. The violent reproduction of bounded identities shows stability, hiding liminality; shows certainty, hiding doubt; and shows stickiness, hiding mobility (Sjöberg, 2012). Queer theorizing of the liminality involved in unstable sex/gender identities shows that even that which is presumed to be the most primordial (sex

identity) is really liminality hiding under supposed definition (Sjoberg and Weber, 2014). Translated to thinking about inhabitability, this theorizing suggests that the apparent safety of inhabitable space hides liminality and uncertainty, and perhaps danger, under its supposed (empirical and normative) clarity (Haritaworn *et al.*, 2014). As such, “all the repressive and reductive strategies of power systems are already present in the internal logic of the sign,” such that “violence is an inevitable byproduct of signification” (Baudrillard, 1981).

In this way, not only do traditional standards of knowledge make invisible their own impossibility, they make invisible their own violence. The raced, sexed, and classed impacts of that masking and the recursive enactment of the standards despite (and at the expense of the visibility) of those impacts continue. Baudrillard suggests a corrective to this break between signs (standards) and referents (the fantasy of the objective existence of “good scholarship”) (Baudrillard, 1973). He argues that:

only ambivalence, as a *rupture* of value ... sustains a challenge to the legibility, the false transparency of the sign ... questions the evidence of the use value of the sign (rational decoding) and of its exchange value (the discourse of communication).

(Baudrillard, 1981)

This ambivalence, Baudrillard argues, “*brings the political economy of the sign to a standstill*”; it dissolves the respective definitions of symbol and referent” (Baudrillard, 1981).

I see this as true, parallel to Agathangelou and Ling’s analysis of the practice of global politics in the scholarship of global politics. As I mentioned, there is a risk of the emptying of signification of IR research programs. Above, I discussed it in reference to the democratic peace, but I should mention that it is not only the democratic peace that risks empty signification. It is also realism (how is that related to the real?), liberalism (which can be deeply conservative), constructivism (which might be everything to everyone or nothing to no one), and a number of other common terms and self-identifications that scholars in IR use to delineate categories. Those categories have become more signifiers of positionality than descriptors of theoretical, ontological, epistemological, or methodological commitments. The “democratic peace” is a politics of liberal inclusionism and a politics of liberal futurism as well as a theory of states’ propensities to war. This is true on two levels: *both* the assumption that all states approximate a particular form of democracy modeled by and for a privileged few states (analogous to the assumption that “quality” scholarship doesn’t change with the demographic spectrum of researchers) *and* the assumption that the transformation of nonconforming states to conformity is net positive rather than violent.

The analysis above indicates otherwise. *Both* endorsing assimilation and assuming its possibility may be net violent (for discussions of the violences of inclusion, see Haritaworn *et al.*, 2013, 2014). Moving of the signification “knowledge” from any referent to which it was originally tied makes method and

research performances of scholarship. If research is a performance of scholarship, “standards” for research serve to disguise the fantastic nature of knowledge cumulation. While the ontological lack is not unique to democratic peace, the insistent performance of normative and methodological *good* might be. In this performance, there is no space for liminality, uncertainty, change, inadequacy, and failure in structural rather than passing senses. Yet looking beyond the performative discourse of certainty, those are exactly what one finds. This paradox, Baudrillard suggests, can only be cleared by *ambivalence* toward the research program and its truth statements. This is because *condemnation* or *rejection* of the research program and its truth statements *endorses* its assumptions about truth, as well as some of its assumptions about what the international arena is and how it works.

Ambivalence, on the other hand, serves to *rupture* the value of the discourse. Here, it is important *not* to equate ambivalence and silence. Ambivalence is not failing to comment on, or failing to acknowledge the existence of, the research program on the democratic peace. Ambivalence is a technical term from psychoanalysis that describes the fluctuation between wanting one thing and wanting its opposite – between embrace and rejection. In this ambivalence, the possibility of strong acceptance and strong rejection *at the same time* (“I love peace, I love democracy, I love the democratic peace” and “democratic peace implicitly endorses slavery and genocide”) *from the same perspective* has the function of showing the distance between sign and referent (and the emptiness of the signifier), providing a path to demonstrate internal contradiction and exclusionary tendencies. It is not to choose between extremes (“the democratic peace is the closest thing to a law that we have in political science” and “the democratic peace is a false construction based on poor knowledge-claims”) but to endorse both extremes as a method of both disruption and revelation.

Knowing and failure

So, IR’s standards are both impossible and violent, while appearing to be a record for success. It is for that reason this chapter visits scholarly failure in both the traditional and queer senses to make the argument that the impossibility of knowledge accumulation in IR does not slow the constant judgment on whether or not knowledge has cumulated. Arguing that fear of liminality, difference, unrecognizability, and failure make it impossible for scholars to explore the futility and impossibility of their own work, I propose we (individually and collectively) invest in elaborate ruses of standards of knowledge accumulation for the promotion of personal, scholarly, and group security. These ruses are developed to hide the emptiness of the referents of signifiers of knowledge production.

Often, scholars talk about knowledge as *interested* in terms of experience, positionality, and the politics of epistemology (Peterson, 1992; Sjöberg, 2006; Steans, 2013), but do not talk about it as *interested* in terms of a scholar’s self-promotion interests in claiming to have produced knowledge and/or self-esteem

interest in feeling like knowledge has been produced. The discipline tends to treat success in knowledge cumulation and success of scholars as related but separable. One is not supposed to take personally the suggestion that one has failed at producing knowledge, and one's apparent success at producing knowledge being rewarded professionally is not tied to personal feelings of need for approval or validation.

But if all claims to the cumulation of knowledge are fraudulent (that is, knowingly untrue) or fantastic (the result of seduction by standards that are empty signifiers), then those claims take on a different character. It would then be interesting to ask *either* why people make claims to knowledge that they do not believe *or* why people believe their claims to knowledge. Particularly, I am interested in scholars' *motivations for* and *politics of* the claim to knowledge production, with a focus on the role of the search for success and professional recognition. I am personally invested in my research; motivated by passion about its subject matter and ambitions for my career. Those feelings are without, within, and constitutive of the work itself – not separate from it. My pride in productivity, and embarrassment in failure, is reflected in the research – that reflection is *material* if narcissistic in the arguments I am making.

The pride in success and embarrassment in failure depend on being able to identify success and failure, which various disciplinary standards for the production of knowledge purport to outline. Generally, there is an implication that research has failed when it does not contribute to the cumulation of knowledge, and that a researcher has failed when s/he is incapable of producing sustained contributions to knowledge. In my reading of the impossibility of detecting the cumulation of knowledge, though, that would make every piece of scholarship and every scholar a failure.

I think that is true. I just do not believe that it is problematic, that failure is always a problem, or that the idea and implications of failure have been fully explored in epistemology in IR. It is, after all, failure that Baudrillard called for at the end of the last section, in different words – a willingness to drop commitment to and passion for a certain end on the recognition that both that end and its opposite are empty signifiers. Here, I am using the word “failure” in two senses: in the traditional sense of failing to reach one's own ends, and in the queer sense of failing to live up to expectations.

When I say that I am talking about failure in the queer sense, I mean the “queer failure” that Jack Halberstam talks about: failure as not “a stopping point on the way to success” but “a category levied by the winners against the losers” and “a set of standards that ensure all future radical ventures will be measured as cost-ineffective” (Halberstam, 2011: 184). The label of “research failure” (the foil to “research success”) is not a weakness to be overcome, but a category constituted by the “winners” as a demonstration of the “losers” being inferior. Failure as a category in IR scholarship serves to reinscribe and renormalize standards of “research success” which remain unchanged, unchangeable, regressive, and violent. The scholarship that makes unconventional claims to knowledge cumulation (or no claim to knowledge cumulation) not only *fails* but constitutes its researchers as

failures – which becomes recursive when “we tend to blame each other or ourselves for the failures of the social structure we inhabit, rather than critiquing the structures ... themselves” (Halberstam, 2011: 35; citing Kipnes, 2004). In Halberstam’s view, it is the system that privileges success that is the problem, and failing within it is an emancipatory possibility which “dismantles the logics of success and failure with which we currently live” (2011: 2).

Rather than being by definition normatively undesirable, in Halberstam’s view, failure can be normatively desirable. S/he suggests that “under certain circumstances, failing, losing, forgetting, unmaking, undoing, unbecoming, not knowing may in fact offer more creative, more cooperative, more surprising ways of being in the world” (2011: 2–3). This is because:

To live is to fail, to bungle, to disappoint, and ultimately to die; rather than searching for ways around death and disappointment, the queer art of failure involves acceptance of the finite, the embrace of the absurd, the silly, and the hopelessly goofy.

(2011: 186–187)

Declaring, and embracing, knowledge cumulation *failure* (and thus, in traditional terms, research failure) “allows us to escape the punishing norms that discipline behavior and manage human development” (2011: 3). Here, the norms that discipline behavior and manage human development are the fetishization of science, the fetishization of progress, and the establishment and reification of boundaries of what ideas are relevant and what ideas are irrelevant. In embracing failure, and escaping those punishing norms that are as violent in their *inclusion*¹⁰ as they are in their *exclusion* “queer studies offer us one method for imagining, not some fantasy of an elsewhere, but existing alternatives to hegemonic systems” (2011: 89). Here, the alternative to the hegemonic system of claims of knowledge cumulation is *the queer* – not only as sexuality, but as lifestyle, as performance, and as foundation for theorizing.

If scholars find their affirmation in (hollow) confirmations of their claims to knowledge cumulation, a queer politics of failure suggests a different direction. As Halberstam recommends, “rather than resisting endings and limits, let us instead revel in ... all of our own inevitable fantastic failures” (2011: 187). Reveling in fantastic failures, in terms of a queer critique of the fantasy of progressive knowledge cumulation, has two elements: enjoying research-as-failure, and confronting the future given that embrace.

Queer theory suggests guidelines for embracing failure: “failing is something queers do well” – not (only) in the self-deprecating sense of laughing at (my own) flaws, but in the more fruitful sense of exposing the ridiculousness of norms by failing to live up to them. In this sense, queer failure is “a map of the path not taken” to “dismantle the logic of success and failure with which we currently live” (Weber, 2014b). “Failing” to meet expectations and being fine repudiates the salvation narrative that accompanies the “right” rules and norms (Weber, 2014b).

The exposure and analysis of queer failure denaturalizes the coherence of knowledge-production performances to show the vapidness inside, and argues that the only way the performance of IR can truly be understood is liminal, transitional, and vulnerable (Butler, 1990, 1993). With Halberstam, I suggest the replacement of “all-encompassing global theories” with those “subjugated knowledges” which have been “buried or masked in functional coherences or formal systemization” (Halberstam, 2011). I am interested in thinking about the failures of successes and the successes of failures – both in traditional terms and in queer terms. That would involve looking for not only “successes” and “failures” (and interchanging them) but looking at researchers’ stakes in each characterization, personally and professionally, as constitutive of how work comes to have those labels attached to it.

The democratic peace research program has been called a failure before (Brown *et al.*, 1996). It has been accused of failing to produce a coherent *explanation* for why democracies are less likely to make wars against other democracies, for failing to distinguish democracy as the causal factor in the peace among democratic states, and/or for failing to provide enough empirical evidence to support the contention that democracies are less likely to fight each other (e.g., on the explanation point, see Bueno de Mesquita *et al.*, 1999: 791–807; on the causal distinction point, see Layne, 1994: 5–49; on the statistics problem, see Chan, 1997: 59–91). These characterizations, however, imply that failure is a bad thing that is avoidable. In so doing, they imply that the standards to which they hold the democratic peace are both possible to meet in a concrete sense and not illusory in an abstract sense. In these discussions, the democratic peace has failed *where it should have succeeded*. Thinking of knowledge cumulation as both traditional and queer failure provides a different perspective.

It suggests that the *expectations* of success in knowledge cumulation for the democratic peace research program (or, indeed, any research program in IR) are impossible to meet. There are normative and political problems *both* with the claim that it is possible to meet those standards and with various claims that some research meets them while other research falls short. Acknowledging the failure of the democratic peace to accumulate knowledge frees the research program from these totalizing moves that do violence both to research/researchers within the research program and to those scholars, practitioners, and “research subjects” outside of it. Such an acknowledgment requires divorcing the worth of the research and researcher from success at knowledge cumulation – that is, deconstruction of the foundational assumptions under which the research was undertaken.

That said, the suggestion that research that *fails* at knowledge cumulation is worthless is short-sighted, incomplete, and impossible. It is impossible because there is no research that does not fail at knowledge cumulation. It is short-sighted and incomplete because it ties value to an intellectual ideal rather than exploring other normative, political, and even psychological sources of worth or value. Indeed, exploring those other sources is the only possible defense of *doing* IR research – given the inevitable failure of knowledge cumulation. I suggest that

embracing that failure is a condition of possibility for such exploration, for creativity, and for the continuation of IR theorizing, inside *and* outside of the democratic peace research agenda.

Productive fantasies

So, if IR research generally and the democratic peace research program specifically is a failure, why research and write? If it is an elaborate sham, why do the work? Why write this chapter? And why would the other authors in this book write theirs? It is my argument that the ways of thinking that make *necessary* the sham of (false) claims to knowledge cumulation are reliant on the (false) dichotomization that IR scholarship is valuable if it contributes to knowledge cumulation and not if it does not. If that (false) claim were true, then the suggestion that both claims to knowledge cumulation and ruses of knowledge cumulation are *failures* would be damning for the enterprise of IR inquiry.

Instead, I at once embrace that IR is a failure at knowledge cumulation and make the argument to continue IR despite (or because of) that inevitable failure, with acknowledgment of it. Rather than an enterprise of knowledge cumulation, I suggest that IR is a *productive fantasy*. I make this argument for two reasons: first, IR knowledge cumulation can be nothing but fantasy; second, the fantasy of knowledge cumulation can be itself productive.

The first argument that IR knowledge cumulation can be nothing but fantasy is straightforward, given the arguments above. If the performance of standards of knowledge cumulation is a signification divorced from a referent, where the recovery of the referent is conceptually and practically impossible, then knowledge cumulation is and will always remain an empty signifier. The only question is how that empty signifier directs and is directed. I suggest that, in IR, more often than not, it directs and is directed by seduction. In Baudrillard's words, seduction is "that which extracts meaning from discourse and detracts it from its truth."¹¹ What makes discourses of knowledge, of science, and of progress seductive "is its very appearance: the aleatory, meaningless, or ritualistic and meticulous, circulation of signs on the surface; its inflections, and its nuances. All of this effaces the content value of meaning, and this is seductive" (Baudrillard, 1991 [1979]).¹² Therefore, if there could be an interpretive discourse of knowledge cumulation that reached truth value, that truth value would be self-defeating, since "the meaning of an interpretive discourse, by contrast, has never seduced anyone" (Baudrillard, 1991 [1979]). This is the fundamental contradiction, in Baudrillard's terms, that makes standards for knowledge cumulation in IR internally impossible. He explains: "every *interpretive discourse wants to get beyond appearances*; this is its illusion and fraud. But getting beyond appearances is appearance, and is hence subject to the stakes imposed by seduction, and consequently to *its own failure as discourse*" (Baudrillard, 1991 [1979]). What is left in/of the failed discourse can only be the fantastic.

This brings me to the second point, that the fantasy of IR (and even the fantasy of knowledge cumulation in IR), even if it is a failure, can be productive.

I rely on literary theory to suggest that fantasy produces both sensory identification and normative grounding. I use these tools to make the suggestion that the sensory and normative value of the fantasy of knowledge production is supplemented with the intellectual value of *discourses of IR* – not as a path to knowledge, or truth, but in themselves. Jim Casey discusses fantasy as “itself postmodern” (Casey, 2012) in a way that stresses the incommensurability of human existence,¹³ representing the unrepresentable with self-conscious recognition of the paradox. While all IR is situated in this way, the explicit characterization of IR as fantasy “displaces not only the centrality of order and meaning, but also challenges the stability of the traditional form and the primacy of the dominant narrative” (Casey, 2012: 117) – enabling a move from “problems of knowing to problems of modes of being” (Casey, 2012, citing McHale, 1987: 10). This explosion of narrative boundaries and the intellectual “paratext” it produces provide a path toward (the seeming paradox of) productive fantasy *acknowledged as fantasy* (2012: 121).

Fantasy is not necessarily insidious; its normative value is related to the normative value of the fantasy experienced or performed, and to the acknowledgment of a particular behavior or set of behaviors as fantastic (Berlant, 2000).¹⁴ IR’s cheap thrills of knowledge production are only cheap when judged by a particular set of knowledge-claims, the impossibility of which created the predicament. Freed to be fantasy, and to engage fantasy, the practice of IR can be a cheap thrill in the best sense of the word. The sort of fantasy I am interested in is the sort that Joan W. Scott explores (Scott, 2011). Scott suggests that fantasy is useful to undermine the notion of psychic immutability or fixed identity, and therefore not only a useful tool for analysis of social phenomena, but a necessary part of any social analysis. The key property of fantasy in literary theory is non-signification – the sign does not portend to have a referent (Jackson, 2002). Instead, “literary fantasy is produced within, and determined by, its social context” (Scott, 2011: 3) where it “characteristically attempts to compensate for a lack resulting from cultural constraints: it is a literature of desire, which seeks that which is experienced as absence and loss” (2011: 5). It is therefore directly a “literature of unreality” (2011: 10) of narrative qualities and narrative effects, where one object does not stand for another (2011: 42).

A literature of unreality for IR would be a scholarship of imagination – imagination of possibilities, imagination of worlds, and imagination of knowledge. Such imagination would not be limited by current performances of disciplinary standards, current disciplining exclusions, or current power structures in the field or in the global political arena. A literature of unreality addressing democracy, war, and peace might imagine multiple realities: realities where people are free from constraint in making decisions (Hirschmann, 2003); realities where people’s decisions are made collectively *and* functionally (Wendt, 1994); realities where peace is the absence of war (Dixon, 1994); realities where peace is associated with the positive provision of needs (Galtung, 1969); realities in which there is a functional world government (which is rarely discussed by political scientists or policymakers); realities in which government is not needed to produce

order (Rosenau and Czempel, 1992) ... the list could go on. This critique resembles in some ways the *Zooming In Zooming Out* articulated by Ish-Shalom in Chapter 8. In each instance the goal is to get away from the pernicious effects of assuming that one objective existence constitutes reality for everyone and that an idea such as the democratic peace can transcend different points of view.

The list of imagined realities from above conveys *productive fantasies* of democracy and peace – simply imagining them provides ideas and feelings for the production of a community of thinkers. Knowledge *cumulation* does not have to be possible for *sensory* knowledge to be produced. One can *feel*, imagine, and sense “democratic peace” in ways parallel to but also unrelated to the substance of the current research program. In addition to the making of productive fantasies about democracy and peace, I suggest that the fantasy of knowledge cumulation *itself* can be productive, *so long as it is recognized as a fantasy*. A fantasy of knowledge cumulation allows a depth of intellectual exploration of particular subjects that might not otherwise be possible. A fantasy of knowledge cumulation produces the social relationship of scholarly communication which can be more than a sum of its parts. A fantasy of knowledge cumulation produces college professors who work in a world of liberal education, encouraging students to question the very foundations their work has come to embrace. A fantasy of knowledge cumulation creates space for scholarly self-indulgence – *enjoying* doing the research, seeing one’s name in print, being cited, and being engaged. This is true for researchers across IR, including in the democratic peace research program. But it inherently moves away from hegemonic imposition of neopositivist standards identified by Jackson in Chapter 1. Instead, a full range of epistemologies would be welcomed into the discourse.

Failing to acknowledge that knowledge cumulation *is* a fantasy destroys almost all of these productive potentials. If *success* in knowledge cumulation is necessary, often depth of exploration of particular ideas is sacrificed in favor of critique between scholars focusing on remaining failures of the research (what is left unknown). If scholars maintain a personal and intellectual identity stake in the success of knowledge cumulation, pride and desperation can get in the way of the social relationship of scholarly communication. If researchers *endorse* the notion that knowledge can be cumulated, we are less critical voices for our students in classrooms. And if the point of “scientific” inquiry (into the democratic peace or elsewhere) is a noble, outside-self, objective cumulation of knowledge, self-indulgence is defined as not only failing (the bad kind) but also normatively evil. If that lofty goal of “helping everyone know” disappears, then there is space for recognizing the possibility that self-indulgence is a legitimate warrant for doing (and publishing) IR research, on an intellectual level as well as on a psychological level.

Arguing with a ghost

Endorsement of the continuation of IR despite the (necessary) failure of it as a knowledge production enterprise is a politics; a politics that can be engaged if

not known. Key to this politics is an embrace of homelessness, liminality, failure, and the joy of comfort in those things. It is the *desire* for those, intellectually and personally, that fuels advocacy for the recognition of a non-cumulative discipline of IR, and for its perpetuation.

That position, however, is not without its complications. For example, the embrace of intellectual dialogue, depth, constant critique, and self-indulgence does not erase the risks of (and manifestations of) the normative problems with the performance of the research to begin with that are mentioned earlier in this chapter. IR research is both *evil* and *fun*; both *problematic* and *enjoyable*; both *good* and *bad*; both *indulgent* and *altruistic*. It is hybrid.

There is also a level of incommensurability in this discussion. If the suppositions in this chapter make sense, then it is arguing with a discipline that does not exist – arguing with a phantom – which either never existed or no longer exists but does not know it.

Perhaps, though, as one of my mentors Hayward Alker argued, the substance is *in* the argument, rather than in its productivity for knowledge cumulation (or lack thereof) or in its result. Hayward suggested that “focusing on narrative scripts and their underlying plots and associated transformational grammars ... helps reconstitute international relations within the dialectical-hermeneutic tradition as a reconstructive but fallible science of human possibilities” (Alker, 1996: 273). In this view, IR is not knowledge cumulation (or lack thereof) but “a performable repertoire or grammar of arguments and counter-arguments” (1996: 52). Alker argues that disciplinary production can/should be a “controversy-based path” to understanding (1996: 53).¹⁵ With Thomas Biersteker, Alker wrote that “it is the sharing, the interpretation, and the principled opposition of these often antagonistic approaches ... that truly constitute the global inter-discipline of International Relations” (Alker and Biersteker, 1984).

Hayward was a believer in knowledge cumulation in IR; I, obviously, am not. But Hayward’s methodological suggestion is useful for both sides of that debate. *With* Hayward’s ghost, I argue for an argumentative dynamic of IR, both substantively and epistemologically. I argue *with* Hayward’s ghost about whether or not the IR with which we both engage is itself an apparition. But that is an argument that we can have *as argument* rather than as *product*; safe because it (like IR) is always and already failed but still *there* to engage. It is my contention that the closer you look at knowledge cumulation in IR, the less you see – but that by giving up pretensions to knowledge cumulation, it is possible to see IR differently.

Notes

- 1 This work is a time-series analysis based on a methodological critique of the “static assumptions” in the statistical methods of leading democratic peace theorists, particularly inasmuch as they cannot account for change over time.
- 2 See Foucault (2003) where discipline is the operation of power which regulates individual behavior within a social body through the regulated organization of activity, time, and space. See discussion in Edkins (1999). For discussion in specific reference to feminist IR, see Steans (2003: 428–454).

- 3 *Politics and Gender* (2011: 573–604), edited by Jennifer Lobasz and myself with contributions from J. Ann Tickner, Carol Cohn, Valerie M. Hudson, Annick T.R. Wibben, Lauren Wilcox, and myself.
- 4 We do not have reliable statistics in IR, but minority underrepresentation is a well documented trend across the social sciences.
- 5 King *et al.* (1994) suggest that the qualitative/quantitative divide should be dispensed with, because “all good research can be understood – indeed, is best understood – to derive from the same underlying [positivist] logic of research,” effectively excluding the post-positivist “side” of the positivist/post-positivist divide. But even the space for doing qualitative research *within* positivist research logics has needed to be defended, e.g., Monroe (2005), Elman and Elman (2003), and Bennett and Elman (2007: 170–195).
- 6 The institutions that support them, according to the authors, include global capitalism, the neoliberal state and its market, the patriarchal family, complicit knowledge construction in the academy, and an ontology of fear and property.
- 7 Statistics on these exclusions are being gathered and available for sharing on request.
- 8 This reading is based on an interpretation of Bhabha (1994), who emphasized the ambivalence and hybridization of the colonizer/colonized relationship – given the lack of absolute rejection of the colonizer, the colonized could not be treated as purely silent and purely oppressed. Bhabha’s inadequate attention to the power differential inherent in that hybridization creates an impossibly incomplete interaction. Hybridity, then, needs to be theorized with attention to power and the violence around that power.
- 9 See the “Murderous Inclusions” special issue of the *International Feminist Journal of Politics* 15(4) (2013).
- 10 See, e.g., discussion in the “Murderous Inclusions” special issue of the *International Feminist Journal of Politics*.
- 11 In other words, Baudrillard (1981) is claiming that Lacan reversed manifest and latent discourse – instead of latent discourse preaching truth to manifest discourse, manifest discourse preaches falsehood to latent discourse – seducing it away from truth/reality.
- 12 That is why “we needn’t search in some beyond, in a *hinterwelt*, or in an unconscious, to find what diverts discourse” (Baudrillard, 1991 [1979]).
- 13 In Lyotard’s terms (1984: 81).
- 14 Berlant (2000) finds the normative value of fantastic sentimentality in the politics of its deployment.
- 15 Note that the next words in Hayward’s text are “knowledge cumulation,” on which, as discussed directly below, Hayward and I disagree. I discuss this in some detail in Sjöberg (2011).

Conclusion

Different standards for discovery and confirmation

Annette Freyberg-Inan, Ewan Harrison, and Patrick James

IR and the philosophy of inquiry

This volume began with observations about a cacophony with regard to the philosophy of science, or more broadly of inquiry, in the context of IR. A rapidly expanding research community seems to lack consensus on the way forward for the discipline. This is coupled with a frequent total absence of awareness of issues in relation to the philosophy of inquiry. In response to this situation, the contributors to the present volume have been asked to think about the major issues at stake, such as the nature of science and criteria for progress. This has set the stage for drawing up a “road map” of philosophy of inquiry in IR in the chapter at hand.

After a review of competing and complementary points of view from the book’s chapters, both about matters in the abstract and in application to the democratic peace, the key concept for reading the map presented here will be developed: *sociable pluralism*. The concept of sociable pluralism entails inclusiveness in the logic of discovery and more exclusiveness in the logic of confirmation. Thus the debate which sets the stage for this volume and was presented in its introduction is resolved in two stages. The first urges that any and all approaches should be welcome within the logic of discovery, with the most efficient allocation of scholarly resources to be determined through experience. The second stage favors a soft version of neopositivism in the logic of confirmation. Confirmation is here taken to mean not that empirical claims can be somehow definitively proven true, but rather, following a falsificationist logic, that they can be left to stand as long as serious attempts to refute them have been undertaken yet not succeeded. The version of neopositivism advocated is soft in the sense of recognizing the merits and implications of social constructivism: we study a social world which responds to our efforts to grasp and navigate within it; our subject matters are inherently dynamic and leave room for human agency, which also raises the need to reflect on our normative commitments; and as scholars we cannot lay claim to a view from nowhere but only to standards intersubjectively negotiated, again in social context. These standards must then be judged by their usefulness for our research goals and cannot lay claim to neutrality, or truth.

Table 10.1 summarizes the answers provided by the chapters in the volume to the three motivating questions posed at its outset:

Table 10.1 Summary of responses to principal questions

Chapter	What criteria exist, and which should be advocated, for evaluating contributions to knowledge?	How can we best utilize those criteria for evaluating past and future theoretical contributions?	What are the implications of these criteria for progress in International Relations?
Jackson	Factual, explanatory, impersonal, and detached in a specific sense; systematicity; publicity; (this-) worldliness	Follow best practices in our domain among the following choices – neopositivism, analyticism, critical realism, and reflexivity	Neopositivism, due to greatest longevity and institutional support, holds an advantaged position; we should move away from seeing only this approach as scientific
Wight	Objective knowledge; reliable guide to action	Establish ontology and methodology, in that order; think of theories as socially constructed maps rather than mirrors; rely on diagrammatic form; obtain validity through complex relational consideration of multiple explanatory components	Accept science and reject relativism; seek relevance by minimizing effects of turgid prose, specialization, and quantification; implement scientific realist treatment of unobservables; research should remain autonomous from the larger social domain
Knutsen	Public conversations; expanding range of substantive issues addressed	Retain connection to real world in seeking insights	Problem of theoretical overload as a result of proliferating schools of thought, with less focus on major events; inversion of method over substance; free speech is crucial and explains why IR remains primarily Western
Freyberg-Inan	Theories should be judged by multiple validity criteria and by their utility for accomplishing practical goals, which in turn demands a recognition of the social and political nature of research and the researcher's normative involvement	Search for theoretical integration should be abandoned in favor of pluralism, openness, and the recognition of complexity; bias in IR in favor of hypothesis-testing and grand theorizing should be overcome	Progress depends on intersubjective agreement on standards for evaluation, the practical usefulness of social science research, and the moral sensitivity of researchers

Chernoff	<p>Simplicity; explanatory unification; range of phenomena explained; empirical adequacy; falsifiability; identification of causal mechanisms; identification of true causes; depth of causes; supportiveness of counterfactuals; the ability to impart understanding; predictive accuracy; comprehensiveness (or the ability to exclude alternative explanations); methodological conservatism</p>	Practice limited pluralism; obtain agreement on criteria prior to research	Need to identify a contrast class of answers; be extremely clear in formulating and explicating the central focus and meaning of any question; focus on precise question to be debated and criteria for decision; attenuated pluralism is best – questions vary in terms of what methods are preferred
Hayes and James	Logical consistency; identify causal mechanisms; comprehensiveness in terms of linkage type	Shift to diagrammatic exposition of theorizing; identify functional form for causal mechanisms	Combine systemism with analytic eclecticism
Harrison	Efficient allocation of scholarly resources; falsifiability; thorough investigation across levels of analysis	Broaden scope and ambition of existing theories	Identify and act upon gaps in existing theories, based on levels of analysis; avoid falling into lazy consensus
Ish-Shalom	Concepts that are exhaustive, exclusive and operationalized; concepts are vetted normatively	Remain aware that essential contested concepts cannot be objective and neutral in definition; both <i>Zooming In</i> and <i>Zooming Out</i> are required	Ideas of positivism and objectivity remain very alluring and tempting; use of manageable but flawed proxy measurements is an ongoing problem; must re-examine operationalized definitions and see if they are compatible with our moral commitments
Sjoberg	None exist	For existing research programs, identify constitutive silences that create conditions of possibility; find visible and invisible omissions	Just as important to see what we do not learn from analysis, in contrast to what we do learn; established research programs render many subjects unheard; should focus on IR as productive fantasy – include a scholarship of imagination

- What criteria exist, and which should be advocated, for evaluating contributions to knowledge?
- How can we best utilize those criteria for evaluating past and future theoretical contributions?
- What are the implications of these criteria for progress in IR?

The contents of the table reflect the most germane points from much longer and complex arguments in each chapter. In some instances the contents of the table, which answer the principal questions in summary form, have been paraphrased to highlight patterns. On the one hand, the table may underestimate diversity in IR because of the obvious fact that only nine points of view are represented in this volume due to constraints of space. On the other hand, contributions to the volume reflect invitations to accomplished scholars with quite divergent points of view. In sum, it is fair to say that no school of thought with a significant number of adherents in IR is absent from the discussion of progress we engage in here.

This chapter continues in five sections. Sections two through four use the contents of the table to identify points of (dis)agreement. The remainder of the chapter seeks to build on the identified consensus and address the evident disagreements in line with sociable pluralism.

What criteria exist and should be advocated?

Table 10.1's second column reveals the presence of diverse criteria for progress among contributors to the volume. Among the criteria listed, comprehensiveness is the only one to appear three times. Public exchange of views, identification of causal mechanisms, range of explanation, falsifiability, rigor, and predictive value are present twice within the table. Also recognition of normativity is called for explicitly twice, by Freyberg-Inan and Ish-Shalom (and implicitly also by Sjoberg). Quite a few criteria appear only once. All of this is striking in that it shows some degree of convergence among a subset of the chapters, but within more visible limits on agreement. Consensus, as far as it goes, primarily appears either within a neopositivist context or among those who reject neopositivism. One major exception would be agreement across the divide by Jackson and Knutsen on the importance of public debate.

Disagreement becomes more apparent once the single listings are brought into play. Take, most obviously, the example of Sjoberg, with no criteria listed because of a fundamental objection to the neopositivist enterprise. This seems to clash with Wight's argument on the need to hold onto the idea of intersubjective knowledge and with neopositivist criteria throughout the table and foreshadows debates to come regarding philosophy of science and IR. By contrast, Freyberg-Inan supports Wight's criteria for judging the validity of knowledge-claims, yet agrees with Ish-Shalom and Sjoberg that they are intersubjectively constructed and have implications for the sorts of knowledge created that should be critically

examined. This is a soft positivist position that bridges important divides in philosophy of science.

How can we best utilize the criteria?

With a turn to application, the third column of Table 10.1 displays what appears to be a greater degree of agreement. Several items appear explicitly twice or even three times: recognition of (causal) complexity, use of diagrams, a call for pluralism, and rejection of the idea of scholarly neutrality. The first two items follow from one of the meta-themes throughout discussion, in the present volume and beyond, of progress in the social sciences: How is it possible to deal with rapid change and attendant mounting complexity? Wight, along with Hayes and James, urges movement toward diagrammatic exposition in order to cope with ongoing accumulation of causal mechanisms either tentatively supported or under consideration. Visual presentation becomes essential to convey accumulated findings from across levels of analysis and identify directions for further theorizing.

Overall, however, ideas in Table 10.1 about how criteria should be applied to assess past and future theoretical contributions remain quite diverse. Several positions emerge, with key points of (dis)agreement coming into focus through comparison of the advice offered by Chernoff and Jackson. Chernoff advocates a limited form of pluralism. Success demands that agreement on criteria of evaluation be obtained *prior* to research. This attenuated pluralism, as designated by Chernoff, is essential to head off debates over methodology that are unlikely to be settled otherwise. In other words, criteria *can* be agreed upon and, in turn, determine methods for a given research question. Jackson, by contrast, argues against uniformity in criteria. He favors following the best practices in any given domain among the four he identifies: neopositivism, analyticism, critical realism, and reflexivity. This position seems to be fully consistent with Chernoff only in the domain of neopositivism; reflexivists, in particular, might object to the idea of an *a priori*, intersubjectively identified set of criteria for contributions across the board.

Some expositions from the table are more in line with Chernoff, while others echo Jackson. The review that follows is not, perhaps, a cacophony as foreshadowed in this volume's introductory chapter. Instead, it is more like a duet out of phase – two basic tunes that seem to remain out of harmony. Ideas in proximity to neopositivism and those distant from it are reviewed in turn and reveal opposing points of view.

With his call for broadening the scope and ambition of theories, Harrison is consistent with the idea that such entities can be compared productively to each other. This is essential to neopositivism but comparison remains flawed in practice. Failure to develop Lakatosian (1970) concepts associated with falsificationism into fully operational form opens the door for exponents of a given research program to rest on their laurels, at least to some extent, because it becomes too easy to claim progress. For example, consider the fact that *every* case included in

Elman and Elman (2003), the most authoritative collective effort based on the MSRP, is affirmed as progressive. Harrison argues in favor of levels of analysis as a means toward assessing comprehensiveness and averting the problems of non-measurement that go with largely rhetorical application of the tentatively specified framework from Lakatos. He identifies the democratic peace – an apparently great success story – as problematic because it focuses so narrowly on interstate dyads. With a turn to the level of the international system, Harrison argues, it is much more likely that knowledge will be gained, with otherwise diminishing returns to be expected from dyad-year oriented research on the democratic peace.

Wight effectively operationalizes critical realism and lays out how competition among theories should work. He calls for establishment of ontology prior to methodology, diagrammatic representation of theories, and for obtaining validity through complex relational consideration of multiple explanatory components. Wight's call to see theories as maps rather than mirrors of reality departs from a purely representational view of the empirical world, but remains relatively close to the neopositivist ideal type because the mapping exercise is to be conducted in tandem with the other steps enumerated a moment ago.

Hayes and James emphasize diagrammatic exposition and see it as the way to obtain a genuine sense of progress for any given research program through a process of comparison. Their implementation of systemism stands as a quite explicit rejection of the sociological model of science from Kuhn (1970), with commensurability of theories obtained through diagrams that can include competing and complementary causal mechanisms. Moreover, Hayes and James note long-term importance, if short-term elusiveness, with regard to designating functional form in causal mechanisms. This degree of specificity – presenting a causal mechanism as incremental or otherwise (i.e., $y=f(x)$) – enhances falsifiability and attendant scientific value.

More distant from neopositivism are the agendas advocated by Jackson, Freyberg-Inan, Knutsen, Ish-Shalom, and Sjoberg. Jackson, as observed already, argues in favor of pluralism, with respective approaches permitted and even encouraged to differ from each other through self-designation of criteria for progress. Freyberg-Inan advocates pluralism with respect to theory development, but supports limited pluralism on the basis of intersubjective agreement on criteria in the domain of theory testing. Based on an exegesis of the history of IR, Knutsen reaches the conclusion that maintaining a connection to the real world is the effective way to obtain insights from theorizing onward. A dialectic exists between historical events and theorizing; criteria for evaluating accomplishments naturally evolve as a result and do not reflect any immanent and intersubjective standard.

Ish-Shalom concurs on the inherent subjectivity of criteria for knowledge, urging scholarly awareness that concepts of genuine interest are contested and cannot be neutral and objective in definition. Ish-Shalom's idea of *Zooming Out* – looking at the moral implications of theorizing – is essential and constitutes a basic departure from neopositivism, which is supported also by

Freyberg-Inan. *Zooming Out* serves as an example of best practices for reflexivity in particular.

Sjoberg takes rejection of the possibility of neutrality a step further and focuses on constitutive silences for existing research programs. From a reflexive point of view, well-placed and highly respected programs such as the democratic peace can end up hiding more than they reveal. Questions from outside of the program of research, which might be posed by those in marginalized positions, could be highly informative when asked – but unfortunately these queries are ruled out of order. This idea parallels, but goes beyond, Ish-Shalom's concept of *Zooming Out*. Ish-Shalom calls for normative vetting of the concepts that make up theories, while Sjoberg sounds the alarm regarding what may be rendered unheard by a highly disciplining program of research.

With its convergence toward measurement that is most amenable to quantification, the democratic peace becomes problematic in particular. From the standpoint of those who would demand a wider range of theorizing that entails normative reflection, the democratic peace is narrow and unconvincing in ways beyond those anticipated by Harrison earlier in a neopositivist context. Put simply, all of this thinking is many leagues away from Chernoff and quite close to what Jackson would call for with regard to the adoption of best practices in the context of a reflexive approach.

What are the implications of the criteria for progress in IR?

Table 10.1's fourth and final column suggests a "glass half full, half empty" situation with regard to implications for IR. Expositions identified with neopositivism tend to take a more positive view of the progress achieved in IR. Those looking at IR from a relatively critical perspective, by contrast, are inclined to be more negative about the field writ large. Details from both sides prove revealing and help to set the stage for an ensuing discussion of potential for synthesis.

Wight urges that the field accept science and reject relativism. While that looks at first sight like a direct throwing down of the neopositivist gauntlet, Wight in fact develops a critical realist take on criteria for progress. Clarity is important above all to preserve relevance; it is essential to minimize the effects of turgid prose, specialization, and quantification. Such advice could be offered in equal measure to exponents of statistical methods and post-modernism, obviously at odds with each other with regard to the philosophy of inquiry. Wight also urges implementation of a scientific realist treatment of unobservables. This point is reinforced by Hayes and James, who call for a combination of analytic eclecticism and systemism to advance the study of causal mechanisms. Wight, moreover, sees a priority beyond instrumentalism in establishing cause and effect relations; where unobservables are involved, predictive accuracy is a prelude to, not a substitute for, establishment of causal mechanisms.

Implications from Chernoff focus mostly on the strategy and tactics to be pursued by a neopositivist IR. A contrast class of answers needs to be identified

for any research problem. This means that a given explanation is evaluated not just in absolute terms, but also should be compared with alternatives to assess its value. To achieve progress, IR must formulate and explicate the central focus and meaning of any research question. This achievement, in turn, facilitates agreement on criteria for a satisfactory answer to a given question. Once properly identified, with criteria for assessing responses in place, questions can be expected to vary in terms of what methods are preferred to answer them. Thus an attenuated form of pluralism, which allows for diversity in methods, is recommended by Chernoff for IR to obtain maximum progress.

Harrison also offers practical advice for IR in the neopositivist tradition. Criteria for progress imply that scholars should identify and act upon gaps in existing theories, notably those that pertain to incomplete treatment regarding levels of analysis. Only in that way can intellectual laziness, epitomized by premature consensus, be averted. This argument from Harrison is quite commensurate with Hayes and James on the subject of causal mechanisms, that is, exploring a full range of potential connections in any system. In the language of systemism as conveyed by Hayes and James, Harrison correctly identifies a lack of activity among democratic peace scholars in terms of (1) the macro-level and (2) transmissions back and forth between micro- and macro-levels. Thus the democratic peace is overwhelmingly dyadic and too readily accepted as the full story regarding regime-type and conflict. The democratic peace, in its present incarnation, is reductionist, which leads Harrison to identify priorities for theory building and testing that include the system (i.e., macro-) level.

Implications from criteria that generally are in line with neopositivism create specific priorities for IR, as per above. Critics, however, see the predominance of neopositivism itself as a problem. This point, made explicitly by Jackson and Ish-Shalom, is the only one to appear more than once as an answer to the question about implications for IR. It is fair to say that the point also is strongly implicit within the chapter by Sjoberg.

Due to its longevity and institutional support, neopositivism holds an advantaged position. Jackson urges that IR move away from a sense of scientific progress that is nested fully within neopositivism and tends to crowd out other points of view. Instead, as pointed out by Ish-Shalom, genuine progress will depend on resisting the ideas of positivism and objectivity, which are very alluring and tempting. In particular, concepts taken for granted in neopositivist research programs need to be re-examined to determine whether they are compatible with researchers' moral commitments. Given neopositivist quasi-hegemony in IR, it is quite possible that any number of basic concepts are in need of review.

Sjoberg reinforces that argument with a more encompassing critique of IR. The focus is on its exclusive character – a result in part of the long-term dominance of neopositivism. It is important, according to Sjoberg, to consider what is *not* being learned from the predominant types of analysis now in place. What subjects, for instance, have been rendered unheard under current conditions? The answer is “too many” and notably those of interest, Sjoberg observes, to

marginalized populations and scholars concerned with their points of view. The overarching implication for IR from the criteria identified by Sjöberg is clear: engage in productive fantasy, which means expansion to include a scholarship of imagination. In this way the narrowing effects of neopositivism, with its idea of scientific progress based on an objective world that is designated by the powerful within it, may be limited or even reversed.

Freyberg-Inan once again occupies a middle ground by stressing the need for intersubjective agreement on standards for evaluation, as do Wight and Chernoff, but also the criterion of practical usefulness of social science research and the need for moral sensitivity of researchers, as do Ish-Shalom and Sjöberg. In this manner, her position develops the synthesis we propose in the following section.

Sociable pluralism: a quest for synthesis

Why is neopositivism so popular? Jackson and other critics cite the long-standing presence of this philosophy of science tradition, which has produced institutional and financial support for the type of research it purveys. So basically, neopositivism benefits from inertia. A causal mechanism consistent with this argument can be identified quite readily, specifically with regard to the staying power of the status quo. Within the study of decision-making, a great deal of evidence supports the existence of status quo bias. A wide range of studies suggest that once an option is identified as the status quo, risk aversion, among other traits, causes people to place value on the current state of affairs that is not based strictly on “hard data.” Evidence that supports the idea of a status quo preference ranges from interview-based case studies to experiments (Huang and James, 2015; Samuelson and Zeckhauser, 1988). Thus one possibility is that the favored position of neopositivism in IR reflects nothing more than arriving on the scene ahead of potentially more interesting competitors.

Consider an alternative argument, based on a time-honored theorem about decision-making, for neopositivist predominance.¹ Condorcet’s Jury Theorem will be put forward here, for better accessibility, in narrative rather than mathematical form. Imagine people tasked with voting independently for one of two options.² One of the underlying options is correct and each voter is assumed to decide with a probability higher than 50 percent to select the right option. In other words, the judgment of each person is imperfect (i.e., less than 100 percent accurate) but also not perverse (i.e., 50 percent or worse). The Theorem asserts that, as the number of voters increases, the probability of an overall correct decision rises as well. In the polar case of an infinite number of voters, the probability of a correct decision converges to 100 percent.

While no mathematical theorem can be expected to offer a perfect portrayal of reality – perhaps a map rather than a mirror in Wight’s sense would be an appropriate way to consider things here – Condorcet’s Jury Theorem can be enlightening in the current context. If the voters are taken to be scientists, then the ongoing success of neopositivism among them might be viewed as a result of the dynamic put forward by the Theorem. Generations of scientists come and go,

but neopositivism keeps on winning because scientists “vote” with far greater than 50 percent likelihood in the direction that is “correct” in the sense of being appropriate to their endeavor. Thus the Theorem offers an alternative to the staying power of the status quo in explaining the sustained success of neopositivism: scientists simply are going for the choice that suits them best. As their numbers expand, and neopositivism maintains support over and above its rivals, the likelihood of its greater usefulness edges toward 100 percent.

Perhaps, also, the sustained dominance of neopositivism is overdetermined. To some extent, neopositivism obviously benefits from its origins ahead of competing points of view available to the scientific community. Some tendency toward a “herd mentality” among scholars that reinforces neopositivism exists. At the same time, it is difficult to dismiss outright the idea of scientists as independent thinkers who nevertheless keep on deciding in the same way vis-à-vis their commitment to inquiry. This may well be the story of IR as we know it.

How far does the argument in favor of neopositivism go? As will become apparent, we suggest that the figurative vote from above applies to the logic of confirmation, but not that of discovery. Each will be discussed in turn in order to provide the foundation for an efficiency-based model – similar to the one developed by Harrison for levels of analysis in the democratic peace research program – for work within IR as a whole.

Consider first the logic of discovery. If an idea is persuasive or a piece of information seems interesting, do its origins matter? Quite intense arguments already exist about how to go about discovering things.³ However, the strength of critical IR comes through most effectively in a critique of any sort of orthodoxy, from neopositivism or elsewhere, at this stage of reasoning. The quite possibly apocryphal story about Sir Isaac Newton discovering gravity as a result of an apple falling on his head works nicely here. It makes the point that the process through which an idea comes about may lie at least partially beyond human control and that very strict rules for the process of discovery may create more of a Procrustean bed than anything else.

Neopositivism – advocated here in a soft form – comes to the fore with the logic of confirmation. This is essential to steer clear of Russell’s Paradox, to be described in a moment. Without intersubjectively acknowledged knowledge as part of the axiomatic basis of science, it is impossible to move forward. Thus Jackson’s taxonomy of approaches might generate applause at one stage but criticism at another. With regard to the process of discovery, neopositivists look like self-defeating “Bolsheviks” as they attempt to designate adherents of analyticism, critical realism, and reflexivity as “Mensheviks” and drive them out of the world of science. As Jackson describes it, the process of expulsion involves a sleight of hand, in which the alternative approaches are seen as not pure enough to deserve the status of scientific inquiry. However, with regard to the logic of confirmation, the situation looks quite different.

Wight anticipates the argument concerning the logic of confirmation with the following assertion: “if all claims are relative why should policy makers accept any claim other than their own, and if the research is not scientific, why should

they use or find it?” Along those lines, consider Russell’s Paradox (1938 [1903]). A common illustration of the Paradox is a barber who shaves exactly all those men who do not shave themselves. After brief reflection of the two basic scenarios – the barber either shaves himself or he does not – Russell’s Paradox emerges. The statement is a contradiction in terms.

With regard to the logic of confirmation, IR outside of soft neopositivism runs into Russell’s Paradox because it denies the existence of intersubjective knowledge. The Paradox plays itself out quite directly: if there is no intersubjective knowledge, then some standard must exist to permit interpretation of evidence to reach that conclusion. But if such a standard exists, then there *is* intersubjective knowledge. To return to the comment from Wight, just above: with no shared standard for assessment, why should anyone listen to anyone else? All debate simply collapses like a house of cards.

Understanding of the need for a soft neopositivist logic of confirmation seems to exist well beyond expositions from its exponents. Consider, for example, the concept of a “brute fact,” elucidated by Guzzini (2000) in an essay that is very favorable to constructivism and shows no obvious adherence to neopositivism. As opposed to “social facts,” Guzzini identifies brute facts with intersubjective knowledge. Even expositions that otherwise are reflexive include references to what seem like brute facts. Sjöberg, for instance, refers to certain demographics as “underrepresented” in IR. Yet that assertion depends on agreement about the rules of statistics.

All things considered, soft neopositivism in the logic of confirmation seems called for to prevent Russell’s Paradox from rendering the entire project of IR untenable. By contrast, in the logic of discovery the story looks much more like the one told by Jackson and his allies on the inclusivist side of the debate regarding progress in IR. Sjöberg, for example, asserts that the “discipline’s standards for knowledge production are political and performative rather than given, objective, useful, or founded.” This observation cannot be challenged by a neopositivist with regard to the logic of discovery. Ideas to be assessed and confirmed through testing – tentatively and within a properly skeptical frame of reference – can and should come from anywhere. With four paths as identified by Jackson – neopositivism, analyticism, critical realism, and reflexivity – the logic of discovery is even most efficient when it corresponds, at least in terms of underlying values, to Sjöberg’s notion of productive fantasy. There is no reason, at the outset, to assume that the standards for *confirmation* should apply to *discovery*. For the latter, we should embrace all basic types of research identified by Jackson. Efficient allocation of resources in this context justifies the pursuit of all types of research, with results attracting or repelling adherents over time.

Quo vadis?

For IR and presumably other would-be social sciences, the different goals associated with the logic of discovery and the logic of confirmation provide a natural breakpoint and go a long way toward setting boundaries for “what goes?” These

concepts are time-honored in the philosophy of inquiry and can help to identify, as per the title of this section, an answer on behalf of IR to the question “where are we going?” from the standpoint of what path is most promising. Sociable pluralism’s best possible meaning should be clear by now: radically pluralistic and inclusive in the logic of discovery, while soft neopositivist in the logic of confirmation.

Notes

- 1 First put forward in the late eighteenth century by the Marquis de Condorcet, this theorem continues to play an important role in democratic theory.
- 2 For an exploration of the independence condition and other issues related to application of the Theorem, see Austen-Smith and Banks (1996).
- 3 In a classic exposition, Popper (2002 [1935]) delivered a polemic on behalf of deductive versus inductive logic with regard to scientific discovery. The literature generated by this book is vast and continues to accumulate.

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